ESSAYS
BIOGRAPHICAL
AND
CHEMICAL

BY

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PREFACE

THESE Essays on Chemical History and Biography, and on chemical topics, have been delivered as lectures, or published as magazine articles at various times in the course of the last twenty-five years. A little alteration has been necessary to avoid undue repetition, and in some cases footnotes have been added, to correct statements which have been rendered inaccurate by the progress of discovery.

I have to thank the University of Glasgow for permission to reprint the oration on Black; the editor of the *Youth's Companion* for permission to reprint the sketch of Lord Kelvin, 'What is an Element?' 'On the Periodic Arrangement of the Elements,' 'Radium and its Products,' 'What is Electricity?' and 'How Discoveries are Made'; the editor of the *Contemporary Review* for permission to reprint the article on 'The Becquerel Rays'; and the Royal Society, which has kindly granted similar permission to republish the life of M. Berthelot.

WILLIAM RAMSAY.

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THE EARLY DAYS OF CHEMISTRY

In the early days of the world's history, the study of science was unknown. The state of society was insecure; nation was constantly invading nation, and men had little leisure for other pursuits save war and the chase. Yet we find, among those nations which were sufficiently powerful to resist the attacks of their neighbours, and sufficiently prosperous to dispense with invasions of the territory of others in quest of plunder, some attempts to inquire into the mysteries of nature. In some countries, as in Egypt, a leisured class of persons, the priests, urged no doubt partly by a desire for knowledge, partly by a wish to impress the people with a sense of their superior powers, made some progress in what may be called 'natural philosophy,' understanding by that term elementary physics and chemistry. To these they added a considerable acquaintance with astronomy and mathematics.

For practical purposes of life, too, certain of the arts, notably metallurgy and dyeing, which are based on chemical principles, were cultivated. But these were carried on by rule of thumb, and their development was slow. Indeed, they were for the most part in the hands of slaves, the freemen finding it more profitable to engage in commerce, or in administration. The state of Turkey or Morocco, in the present day, gives a good

idea of the condition of life in the centuries before the Christian era, in so far as pursuit of science is concerned. Even with the example of adjoining nations, whose prosperity is in great part due to the attention they have paid to the cultivation of scientific knowledge, the Turks and the Moors display a total lack of interest. Much less, then, could people such as those be expected to show any eagerness in the discovery of Nature's secrets.

Yet from time to time there have been minds who refused to accept the daily drudgery of life as sufficient for their needs. Questions such as: Whence did this world arise? What does it consist of? What will be its ultimate fate? perplexed them, as they perplex us; and in an endeavour to answer questions like these, scientific discovery was begun. Many nations, however, were instructed by the priests of their religion that it is impious to make such inquiries; and it is not until the era of the early Greek civilisation, when the current mythology had ceased to retain its hold on abler minds, that we find any serious attempt to grapple with fundamental problems like those stated. But even among the Greeks we meet with a disinclination to take trouble about matters which were imagined to have little if any relation to human affairs; even Socrates, one of their greatest thinkers, taught that it was foolish to abandon those things which more nearly concern man for things external to him. Plato, who chronicled the sayings of Socrates, wrote in the seventh book of the *Republic*: 'We shall pursue astronomy with the help of problems, just as we pursue geometry; but if it is our desire to become acquainted with the true nature of astronomy, we shall let the heavenly bodies alone.' And he states in another place, that even if we were to ascertain these things, we could neither alter the course of the stars,

nor apply our knowledge so as to benefit mankind. And in Timaeus, Plato remarks, 'God only has the knowledge and the power which are able to combine many things into one, and to dissolve the one into the many. But no man either is, or ever will be, able to accomplish either the one or the other operation.'

Even in the middle ages, the same spirit of content with insufficient observation, and the same disposition to draw conclusions from insufficient premises, is to be noticed. It is difficult for us, in this age when a certain acquaintance with scientific methods of thought, if not with scientific facts, is common to almost every one, to imagine the kind of reply to elementary questions which satisfied our predecessors, even those who devoted time and, one would hope, some powers of mind to a consideration of the subject. Let us take a few examples.

The answer which one of the schoolmen would give to the question: 'Of what are bodies composed?' is thus paraphrased by Le Febure, apothecary to His Majesty Charles the Second: 'If the substance is a body, it must possess quantity; and of necessity, it must be divisible; now, bodies must be composed either of things divisible, or indivisible, that is, either of points, or of parts: a body, however, cannot be composed of points, for a point is indivisible, possessing no quantity, and, consequently, it cannot communicate quantity to a body, since it does not itself possess it. Hence it must be concluded that a body must be composed of divisible parts; to this, however, it may be said that such parts must either be divisible or indivisible; if the former, then the part cannot be the smallest possible, since it may itself be divided into others still more minute; and if this smallest part is indivisible, the same difficulty confronts us, for it will be without quantity, which, therefore, it cannot communicate to a body, for it itself does not possess it,

seeing that divisibility is the essential property of quantity.' The logic is unanswerable, but we are left where we were.

Let us next see what ideas were held by Du Clos, physician to Louis XIV., on the cause of the solidification of liquids. These are his memorable words:

'The reason of the concretion of liquids is obviously dryness; for this quality, being the opposite of moistness, which renders bodies liquid, may well produce an effect opposite to that produced by the latter, to wit, the concretion of liquids.' Again, we have not gained much information by the profound utterance.

One more quotation. It is from a work by Jean Rey, Doctor of Medicine, published in 1630, entitled, 'On an Inquiry wherefore Tin and Lead increase in Weight on Calcination.' He is arguing that 'Nature abhors a vacuum,' a favourite thesis in former days. 'It is quite certain that in the bounds of nature, a vacuum, which is nothing, can find no place. There is no power in Nature from which nothing could have made the universe, and none which could reduce the universe to nothing: that requires the same virtue. Now the matter would be otherwise if there could be a vacuum. For if it could be here, it could also be there; and being here and there, why not elsewhere? and why not everywhere? Thus the universe could reach annihilation by its own forces; but to Him alone who could make it is due the glory of compassing its destruction.'

We must remember, therefore, in studying the early history of chemistry, that not only were facts, familiar to many of us now, wholly unknown; but we must also bear in mind that the point of view from which the early chemists surveyed the phenomena of nature was entirely different from that to which we are now accustomed. It is evident, from the examples quoted, which are not

taken from the writings of those who lived at a very remote time from the present day, but only six or seven generations ago, that our great-great-great-grandfathers differed from ourselves not merely in lack of knowledge, but in the way they regarded the facts which they observed. And it is consequently somewhat difficult for us to adopt their point of view, and to think their thoughts. But we must attempt to do so, if we are to realise the progress of our science.

The progress of the science of Chemistry, indeed, forms one phase of the progress of human thought. The ideas which have been held, however, run in certain channels. They may all be referred to speculations on the nature of matter; but the speculations take different forms. For it may be inquired: What forms is matter capable of assuming? Or, what is the minute structure of matter? Or, what changes does matter undergo? These three questions were for the ancients, as they are still for us, fundamental; and it will be the aim of these essays to endeavour to give the reader some idea of the history of these three lines of thought. We shall see that our present knowledge enables us in some measure to connect these three lines of inquiry by virtue of certain hypotheses; but it will be convenient to treat of each separately, at least up to a certain stage.

THE ELEMENTS

The word 'Element,' in the old days, had a meaning different from that which we now ascribe to it; or, to be more exact, it had two meanings, which were frequently confounded with one another. The suggested derivation of the word indicates one of these meanings; it is that which we usually give it; for, just as 'l,' 'm,' and 'n' are

constituents of the alphabet, so an 'element' was regarded as a *constituent* of substances. From the use of the word by ancient authors, however, it would appear that an element was often regarded as a *property* of matter; and it was evidently supposed that by changing the properties, or in the words of the old writers adding more or less of one or other element to a substance, the substance itself could be transmuted into another wholly different. We shall see examples of the two meanings illustrated later on.

It is probable that the original ideas of elements reached Greece from India. The Buddhistic teaching was that the elements are six in number, namely, Earth, Water, Air, Fire, Ether, and Consciousness. But they are given by Empedocles of Agrigent, who lived about 440 B.C., without the two last; and many disputes arose as to which was to be regarded as the primary one, from which all the others were derived; for even at that remote date, speculation was rife as to the unity of matter. While Thales contended that the original element was water, Anaximenes believed it to be air or fire; and Aristotle did not regard elements as different kinds of matter, but as different properties appertaining to one original matter. Plato, however, evidently considered elements to be different kinds of matter, for he puts these words into the mouth of Timaeus: 'In the first place, that which we are now calling water, when congealed, becomes stone and earth, as our sight seems to show us [here he refers probably to rock-crystal, then supposed to be petrified ice]; and this same element, when melted and dispersed, passes into vapour and fire. Air, again, when burnt up, becomes fire, and again fire, when condensed and extinguished, passes once more into the form of air; and once more air, when collected and condensed, produces cloud and vapour; and from

these, when still more compressed, comes flowing water; and from water come earth and stones once more; and thus generation seems to be transmitted from one to the other in a circle.'

Aristotle attributed to these elements four properties, of which each possessed two. Thus, Earth was cold and dry; Water, cold and moist; Air, hot and moist; and Fire, hot and dry. A fifth element was also conceived by Aristotle to accompany these four; he termed it ὕλη, translated into the Latin Quinta Essentia; and this was regarded by alchemists of a later date as of the utmost importance, for it was supposed to penetrate the whole world. The ceaseless strivings of the alchemists after the 'quintessence' were due to the notion that, were it discovered, all transmutations would then be possible. Yet the word 'Chemistry' was not, so far as we know, in use in Aristotle's time. It is said to occur in a Greek manuscript of Zosimus, a resident in Panapolis, a city in Egypt, who wrote in the fifth century. It appeared to mean the art of making gold and silver; for the title of his work is given by Scaliger as 'A faithful Description of the sacred and divine Art of making gold and silver.' M. Berthelot, who has made a detailed study of ancient Greek, Arabic, Syriac, and Latin manuscripts relating to early chemistry, believes that the attempts to transmute metals arose, not from any philosophical notions regarding the nature of elements, but from fraudulent attempts of goldsmiths to pass off base metals on their customers for silver and gold. One of the earliest manuscripts on record dates from the third century, and is preserved at Leiden in Holland. It was found in a tomb in Thebes in 1828. It is a rough and ill-spelt collection of workman's receipts for working in metals, in which frequent reference is made to an alloy of copper and tin—an alloy which in many respects resembles gold. It is apparently a manu-

script which escaped the fate of most of the Egyptian MSS. of that date; for about the year 290, the Emperor Diocletian commanded that all works on alchemy should be burnt, 'in order that the Egyptians might not become rich by the art [of making gold and silver] and use their wealth to revolt against the Romans.'

But although the idea of transmutation did not arise from such theoretical speculations as Aristotle's on the unity of matter, and on the possibility of converting one kind of matter into another by altering its properties, or in the language of the time, adding or removing more or less of one or other element, yet the later workers did not scruple to use Aristotle's theory in order to make good their case. And for many centuries—indeed until our own time—there have always existed men who devoted their lives to this object.

There was, at the same time, a supposed mystical connection, of Chaldean origin, between the metals and the planets. Thus gold was the sun; silver, the moon; copper, Venus; tin, and afterwards mercury, was associated with the planet of that name; iron, used in battle, had affinity with ruddy Mars; electron, an alloy of gold and silver, and subsequently tin, was Jupiter; and sluggish and heavy lead was the slow-moving Saturn. These analogies were used in casting horoscopes, or predicting the future of those rich and credulous enough to consult astrologers.

At the same time as these fantastic notions were held, many processes of manufacture, involving a knowledge of chemical reactions, were carried on. These will be alluded to later; but it may be noted here that speculation did not take the course of attempting to devise explanations of chemical changes, but was indulged in, as before remarked, with little reference to experimental methods.

The conquest of Egypt by the Arabians in the seventh century put an end for a time to the school of learning of Alexandria, where citizens of all nations met and discussed problems of all kinds. But the spirit of the Græco-Egyptians was too strong even for the fanaticism of the Arabians; the conquered became the conqueror; and an Arabian school of philosophy arose, which carried on the traditions acquired from the Greeks. It has been believed, until M. Berthelot showed the belief to be erroneous, that Latin works which professed to be translations from the Arabic of the eighth and succeeding centuries were really renderings of the ancient Arabian authors. It appears, however, that they are for the most part forgeries, having little if any resemblance to the originals. Thus Geber, said to have been translated into Latin in 1529, is entirely different from the Arabic writings of the real Geber. The historical Geber lived in the ninth century. His comment on alchemy is characterised by strong common sense. It is: 'I saw that persons employed in attempts to fabricate gold and silver were working in ignorance, and by false methods; I then perceived that they belonged to two classes, the dupers and the duped. I pitied both of them.'

About this time, however, an addition to Aristotle's classification of elements was made; and it endured until within the last two hundred years. It evidently arose from attempts to account for the properties of the metals, and the changes which they undergo by heat. These additional 'principles,' as they were termed, were salt, sulphur, and mercury. We read that the noble metals contain 'a very pure mercury,' the meaning being, probably, that they possess a high metallic lustre; while the common metals, such as copper and iron, contain 'a base sulphur,' implying that these metals are easily altered by fire, losing their metallic appearance and changing into

black scales. These principles were later increased to five, by the addition of 'phlegm' and of 'earth.' Fanciful analogies were drawn between the Divine Trinity of Father, Son, and Holy Spirit, the human Body, Soul, and Spirit, and the three principles above-named. Attempts were incessantly made to draw inspiration from such impossible fancies. Thus the volatilisation of mercury, or 'Spirit' as it was sometimes called, was deemed analogous to the ascension of Christ! In fact, there is no limit to the absurdity and folly of the endeavours of the alchemists. Let us hear a list of their processes, as told by Sir George Ripley, who lived and wrote in 1471.

> 'The fyrst Chapter shalbe of naturall Calcination;
> The second of Dyssolution secret and phylosophycall;
> The third of our Elemental Separation;
> The fourth of Conjunction matrymonyall;
> The fifth of Putrefaction then followe shall;
> Of Congelatyon, albyfycative shall be the Syxt,
> Then of Cybation the seaventh shall follow next.
> The secret of our Sublymation the eyght shall show;
> The nynth shall be of Fermentation;
> The tenth of our Exaltation I trow;
> The eleventh of our mervelose Multyplycatyon;
> The twelfth of Projectyon, then Recapytulatyon;
> And so thys treatise shall take an end,
> By the help of God, as I entend.'

These chapters are wearisome and rambling; and it is impossible to gain a single clear idea from their perusal. Indeed it was part of the creed of the alchemists that their secrets were too precious to be revealed to the baser sort of men.

> 'The Philosophers were y-sworne eche one
> That they shulde discover it unto none,
> He in no boke it write in no manere
> For unto Christ it is so lefe and deare:

> That he wol not that it discovered be,
> But where it liketh to his deite:
> Man to inspire and eke for to defend
> Whan that him liketh: in this is his end'—

sang Chaucer, and he told a true tale, for the meanings of alchemical expressions are often undecipherable.

The green lion, the basilisk, the cockatrice, the salamander, the flying eagle, the toad, the dragon's tail and blood, the spotted panther, the crow's bill, blue as lead, kings and queens, red bridegrooms and lily brides, and many more mystical terms which had no doubt some meaning to adepts, were mingled in inextricable confusion.

Moreover, the alchemists made use, not only of fantastic expressions, in order to preserve their supposed secrets from the common people, but they had also a set of symbols, possibly originating from the Chaldean or Egyptian alphabets, by which the substances and many of the processes used were symbolised. While the chief aim of modern science is perspicuity, that of the alchemists was ambiguity and mystery. In many cases they were so successful in preserving their secrets that even modern investigation has failed to reveal them. But there is one grain of comfort; albeit it savours of sour grapes, it is perfectly certain that there was nothing worth revealing; at least nothing which it could profit a modern student of science to know. Where the descriptions have been interpreted, they refer to imperfect methods of doing what we are now able to do with much greater economy and rapidity. As already pointed out, their theory of elements was erroneous; they were, moreover, acquainted with very few pure substances, and had no criterion of the purity of those they possessed; and they failed to realise the existence of gases as forms of matter.

Yet the interminable experiments which were conducted with a view of discovering the 'Philosopher's Stone,' which should convert the baser metals into gold, and the *elixir vitæ*, which should convey undying youth on its happy possessor, led to the discovery of many chemical compounds. The writings of Basil Valentine, reputed to have been a Benedictine monk living in South Germany during the latter half of the fifteenth century, contain a description of many substances, now known as chemical entities, together with the methods of preparing them. In a tract entitled 'The Great Stone of the Ancients,' he gives in detail the properties of ordinary sulphur; of mercury, alluding to the medicinal uses of its compounds; of antimony oxide or 'Spiessglas,' which he conjectures to consist of 'much mercury, also much sulphur, though little salt'; of copper-water, or a solution of copper sulphate; of *lima potabilis* or solution of silver nitrate; of quick-lime; of arsenious oxide; of saltpetre. The last he makes tell its own story: 'Two elements are found in me, in quantity—fire and air; I contain water and earth in less amount; therefore am I fiery, burning, and volatile. For a subtle spirit resides in me; I am likest to mercury—inwardly hot but outwardly cold. My chief enemy is common sulphur; and yet he is my greatest friend, for I am purified and refined through him.' Sal-ammoniac, tartar, vinegar, and above all, numerous compounds of antimony were also described by Basil Valentine, the last in his celebrated work entitled, *The Triumphal Chariot of Antimony*. In his writings, however, he points out that many of the substances he describes have medicinal properties; and his successors, of whom perhaps the best known was Paracelsus, developed this part of his teaching. Yet in spite of his considerable knowledge, he retained belief in transmutation: he also added one to the previously received two principles of

ber and his disciples, namely salt, or, as he terms it, alt of the philosophers'; it is the constituent of matter, hich confers solidity, and which remains after the volatile ercury and sulphur have been removed by heat.

In the first half of the sixteenth century Paracelsus tended and applied the suggestion of Basil Valentine, d founded what became known as the school of 'iatro-emists'—a body of men who taught that the chief ject of chemistry is not the transmutation of metals, it the application of chemical substances to medical es. He adhered, however, to Valentine's theory of the ree principles; but he applied them to the human dy, teaching that the organism itself consists of these inciples, and that disease, owing its origin to a deficiency one of them, is to be combated by its being restored the system. Increase of sulphur, he taught, gives rise fever and the plague; increase of mercury to paralysis d depression; and of salt, to diarrhœa and dropsy. Too tle sulphur in the organism produces gout; delirium is used by distilling it from one organ to another, and so in fanciful theorisings. One of the most fantastic is his tributing the nutrition of the body to a beneficent spirit, med the 'Archæus,' who resided in the stomach, and esided over the function of digestion. But these rious notions have little bearing on the development chemistry. The teaching of Paracelsus, however, had e good effect of directing attention to an important anch of chemistry—its use in pharmacy. And from is time onwards, indeed, up to the middle of last cen-ry, many of the best-known chemists had received a edical training, and the ranks of chemical investigators ere largely recruited from the medical profession.

Although the alchemists, after the beginning of the venteenth century, exercised little influence on the rogress of chemistry, they continued their fruitless

quest. The possibility of transmutation has always been associated with speculations concerning the unity of matter. And although there is little evidence as yet to justify the supposition that all substances are ultimately composed of matter of one kind, still the history of our science contains many accounts of attempts to effect transmutation. One such attempt, in modern times, was made by Dr. Samuel Brown, who claimed to have obtained silicon from paracyanogen, a compound consisting of carbon and nitrogen alone; but subsequent workers failed to substantiate his results. There is, however, no question as regards the honesty of Dr. Brown's work; the only conclusion is that he must have omitted to take sufficient precautions against contamination of his carbon compounds with silicon. There exist at present in France also secret societies, with such titles as 'L'Ordre de la Rose-Croix,' and 'L'Association alchimique de France,' the latter the successor of one named 'La Société Hermétique.' One of the latest of their 'researches' was carried out by 'Maître' Théodore Tiffereau; he professed in 1896 to have obtained compounds of carbon—ether and acetic acid—from the metal aluminium, sealed up with nitric acid in a glass tube, and exposed to the sun's rays for two months. But the attempt to transmute baser materials into gold still holds the field. August Strindberg claims to have produced 'incomplete' gold from ferrous ammonium sulphate; and still more recently Emmens, who, however, disclaims the name of alchemist, states that he has converted Mexican silver dollars into gold, or more correctly, increased the small amount of gold actually present in such coins, by hammering the metal exposed to an extremely low temperature. There is reason to suspect the existence of an element which should resemble both gold and silver; Emmens professes to have made this element, which he names

argentaurum, by hammering silver, and to have transmuted it, by a further process, into gold. He claims, too, that Sir William Crookes has obtained proof, slight it is true, though decisive, of an increase in the quantity of gold in a Mexican dollar, after treating the latter by his process.

We have seen from what precedes that the doctrine concerning elements, held from remote times, was that they were four in number, earth, water, air, and fire. That besides these, there exist three chemical or 'hypostatic' principles, to wit, sulphur, mercury, and salt. In spite of the refutation of such views by the Honourable Robert Boyle, which we shall consider later, they lingered on until the middle of last century, being quoted in almost all treatises on chemistry. Macquer's *Chemistry*, a text-book which obtained a wide circulation in its day, gives the following description of the ancient elements (1768): 'Air is the fluid which we constantly breathe, and which surrounds the whole surface of the terrestrial globe. Being heavy, like all other bodies, it penetrates into all places that are not either absolutely inaccessible or filled with some other body heavier than itself. Its principal property is to be susceptible of condensation and rarefaction; so that the very same quantity of Air may occupy a much greater or a much smaller space, according to the different state it is in. Heat and cold, or, if you will, the presence or absence of the particles of Fire, are the most usual causes, and indeed, the measure of its condensation and rarefaction: for, if a certain quantity of air be heated, its bulk increases proportionately to the degree of heat applied to it; the consequence of which is, that the same space now contains fewer particles than it did before.' 'Air enters into the composition of many substances, especially vegetable and animal bodies; fully analysing most of them, such a considerable quantity

thereof is extricated, that some naturalists have suspected it to be altogether destitute of elasticity, when thus combined with other principles in the composition of bodies.'

After describing some of the physical properties of water, Macquer continues: 'Water enters into the texture of many bodies, both compounds and secondary principles; but, like air, it seems to be excluded from the composition of all metals and most minerals. For although an immense quantity of water exists in the bowels of the earth, moistening all its contents, it cannot be thence inferred that it is one of the principles of minerals. It is only interposed between their parts; for they may be entirely divested of it, without any sign of decomposition: indeed, it is not capable of an intimate connection with them.'

Of earth he says: 'We observed that the two principles above treated of are volatile; that is, the action of fire separates them from the bodies they help to compose, carrying them quite off and dissipating them. That of which we are now to speak, namely earth, is fixed, and when it is absolutely pure, resists the utmost force of fire. So that, whatever remains of a body, after it has been exposed to the power of the fiercest fire, must be considered as containing nearly all earthy principle, and consisting chiefly thereof.' 'Earth, therefore, properly so called, is a fixed principle which is permanent in the fire.' He then goes on to distinguish between fusible or vitrifiable earths, and infusible or unvitrifiable earths, the latter of which are also called absorbent earths, from their property of imbibing water.

Maquer's views regarding fire are as follows: 'The matter of the sun, or of light, the Phlogiston, fire, the sulphureous principle, the inflammable matter, are all of them names by which the element of fire is usually denoted. But it

should seem that an accurate distinction has not been made between the different states in which it exists; that is, between the phenomena of fire actually existing as a principle in the composition of bodies, and those which it exhibits when existing separately, and in its natural state: nor have proper distinct appellations been assigned to it in these different circumstances. In the latter state, we may properly give it the names of fire, matter of the sun, of light, and of heat; and may consider it as a substance composed of infinitely small particles, continually agitated by a most rapid motion, and of consequence essentially fluid.' 'The greatest change produced on bodies, by its presence or its absence, is the rendering them fluid or solid; so that all other bodies may be deemed essentially solid; fire alone essentially fluid, and the principle of fluidity in others. This being presupposed, air itself might become solid, if it could be entirely deprived of the fire it contains; as bodies of most difficult fusion become fluid, when penetrated by a sufficient quantity of the particles of fire.'

An attempt has been made in the preceding pages to show the manner in which the world around us was regarded. People were content to take as true what they were told; in fact, it was regarded as unfitting that the 'mysteries' with which we are surrounded should be too minutely inquired into. Great reverence was paid to tradition; and more attention to the celebrity and personal character of those who advocated certain dogmas than to the evidence in favour of their intrinsic probability.

This spirit is by no means extinct; the vast majority of the human race are content to gain knowledge at second hand. Whether such knowledge is worth having may well be questioned; it is of course impossible that every man should investigate natural phenomena for himself; but it is at least possible to place every child in

the position of knowing, in however elementary a way, how useful deductions have been drawn from observation and experiment, and of emancipating himself, to some extent at least, from the thraldom of intellectual authority.

THE GREAT LONDON CHEMISTS

I. BOYLE AND CAVENDISH

THE country which is in advance of the rest of the world in Chemistry will also be foremost in wealth and in general prosperity. For the study of Chemistry is so closely bound up with our development in all kinds of industry, with the arrestment of disease, and with our success in war, that it is essential to a wealthy, healthy, and peaceful nation. The electrician is dependent on the chemist for the iron suitable for his dynamos; the engineer, for the materials which he uses in his construction; and the scouring, bleaching, and dyeing of the fabrics with which we are clothed, the manufacture of the paper on which we write, and the ink with which we soil the paper; the provision of our food-supply, and the removal of effete matter from our houses; the preparation of our medicines; and the synthesis of the high explosives with which warfare is now conducted; all these belong to the domain of the chemist, and without them we should lapse into the semi-barbarism of our ancestors.

Still, it must be borne in mind that we are far from perfection. No process is so perfect that there is not plenty of room for improvement. There is no finality in science. And that which to-day is a scientific toy may be to-morrow the essential part of an important industry. This is one, though not in my view the most important, inducement to study the science of Chemistry.

To extend the bounds of human knowledge, and in so doing to glorify our Creator, is surely still more an end to be striven after. To quote from the words of Francis Bacon, prefixed by Charles Darwin to his *Origin of Species*: 'To conclude, therefore, let no man, out of a weak conceit of sobriety or an ill-applied moderation, think or maintain that a man can search too far, or be too well studied in the book of God's words, or in the book of God's works, divinity, or philosophy; but rather let men endeavour an endless progress or proficience in both.' Yet the acquisition of wealth and fame will probably now, as it has in the past, appeal more forcibly to the mind of the ordinary man; and we must not despise any inducement, which will lead to the furtherance of the object to be gained, provided the motives are not in themselves sordid.

The study of science, with the express object of securing wealth and fame, is not likely to secure either. The old story of the desire of King Solomon is often fulfilled in our day. Solomon's request was, 'Give me now wisdom and knowledge'; and he was answered, 'Wisdom and knowledge is granted unto thee, and I will give thee riches and wealth and honour.' The reason why an attempt to utilise science for the attainment of wealth often fails is a simple one. It is due to the unfortunate circumstance that the human mind is not omniscient. No man, beginning a research, can know to what it will ultimately lead. It will certainly, if rightly pursued, lead to knowledge; but whether it will bring riches and fame is beyond his ken. There have been, however, researches expressly directed to some specific object, which have succeeded in their purpose; and we shall see later how the discovery of principles which led to the invention of the safety-lamp by Sir Humphry Davy illustrates this. But as a rule, those chemists who have

achieved for themselves immortal fame have striven after the nobler goal—the increase of the sum of human knowledge. It is to the lives of some of those, who have been more or less connected with London, that I ask your attention. May those of us who follow, at however far a distance, profit by their example!

In the olden days, science, as we know it now, was non-existent. The minds of most men who were free from the thraldom of incessant labour were occupied with war or statecraft as a business, and with the chase as a recreation. Those to whom such pursuits, from circumstances or mental habit, were repugnant, found occupation in history, poetry, philosophical discussion, or religion. It is true, speculation on the nature of the world around them was indulged in by some; but they were guided in their views by their opinion rather of what *ought to be,* than what *is.* The attitude of the modern mind is more humble. We no longer believe that we share enough of the creative power to enable us to construct a system of the universe; we are content if we are able, in however modest a way, to interpret nature, and we call to our aid experiment, as a means of questioning nature. We are prompt in communicating our knowledge to others, and we expect their aid and look for their criticism. In former days, the language of mystery was employed. It concealed secrets too precious to be laid bare to the vulgar crowd. 'In those days,' to quote the words of Dr. Samuel Brown,[1] 'the metals were suns and moons, kings and queens, red bridegrooms and lily brides. Gold was Apollo, "sun of the lofty dome"; silver, Diana, the fair moon of his unresting career, and chased him meekly through the celestial grove; quicksilver was the wing-footed Mercury,

[1] Dr. Samuel Brown's *Essays*.

Herald of the Gods, "new-lighted on a heaven-kissing hill"; iron was the ruddy-eyed Mars, in panoply complete; lead was heavy-lidded Saturn, "quiet as a stone," within the tangled forest of material forms; tin was the Diabolus Metallorum, a very devil among the metals, and so forth in not unmeaning mystery.

'There were flying birds, green dragons, and red lions. There were virginal fountains, royal baths, and waters of life. There were salts of wisdom, and essential spirits so fine and volatile, that drop after drop, let fall from the lip of the wonderful phial that contained them, could never reach the ground. There was the powder of attraction which drew all men and women after its fortunate possessor; and the alcahest, or universal solvent and *noli-me-tangere* of essences. There was the grand elixir that conferred undying youth on the glorious mortal who was pure and brave enough to kiss and quaff the golden wavelet as it mantled o'er the cup of life—the fortunate Endymion of a new mythology. There were the Philosophical stone, and the Philosopher's stone; the former the art and practice, the latter the theory and idea, of turning baser natures into nobler; the theory and practice of exaltation. The Philosophical stone was younger than the elements, yet at her virgin touch the grossest calx among them all would blush before her into perfect gold. The Philosopher's stone was the first-born of all things, and older than the king of metals.—In a word, there was an interminable imbroglio of a few of the hard-won facts of nature, a multitude of traditionary processes and results, several very just analogies, some most fantastical notions, one or two profound, but intractable ideas, a haze of philosophical mysticism, and an under-current of fervid religiosity.'

Such conceptions ruled the minds of philosophers, as they loved to call themselves, until the middle of the

seventeenth century. But the practice of interrogating nature by experiment had sprung up, and was soon destined to bear good fruit. Although these notions of matter and its elementary forms lingered on until a much later date, and indeed are not wholly extinct at the present day, they received their first great blow about this time; the first brunt of an attack which was destined ultimately to overthrow them.

This attack was made by Boyle. The spirit in which he approached the hostile ranks is best given in his own words: 'For I am wont to judge of opinions, as of coins; I consider much less in any one that I am to receive whose inscription it bears, than what metal 'tis made of. 'Tis indifferent enough to me whether 'twas stamped many years or ages since, or came but yesterday from the mint. Nor do I regard how many or how few hands it has passed through, provided I know by the touchstone whether or no it be genuine, and does or does not deserve to have been current. For if, upon due proof, it appears to be good, its having been long and by many received for such will not tempt me to refuse it; but if I find it counterfeit, neither the Prince's image nor superscription, nor its date, nor the multitude of hands it has passed through, will engage me to receive it. And one disfavouring trial, well made, will much more discredit it with me, than all those spurious things I have named can recommend it.'

In this spirit the 'Sceptical Chymist, or considerations upon the experiments usually produced in favour of the four elements, and of the three chymical principles of the mixed bodies' was written. In it, the various theories of matter, which, like a river rising in the remotest recesses of time had gathered tributaries as it flowed and presented a formidable flood in Boyle's days, were searchingly criticised. Every postulate was examined;

if possible, experimentally tested; if true, kept; if false, rejected.

Thus, early in the book, we meet with the phrase, long accepted as true, *Homogenea congregare*; that is, 'Like draws to like.' This Boyle disproved by showing that liquids, like alcohol and water, alike in being colourless and transparent, although they mix with each other, may be easily separated by freezing; for, when cooled, the water freezes, leaving the alcohol unfrozen. Here we find the first record of experiments on a subject which, in Raoult's hands, yielded such extraordinarily important results. Another of Boyle's arguments is, that although liquids and gases mix respectively with each other, yet solids show no such tendency, and do not even cohere, except in cases where the cohesion can be explained by the form of the solid, and the consequent exertion of atmospheric pressure.

After making a number of such attacks, Boyle proceeds to consider the hypothesis at that time all-prevalent and universally accepted, of the elements salt, sulphur, and mercury. He opens two distinct lines of attack. His first may be stated thus: If all substances are composed of salt, sulphur, and mercury, and if vegetable and animal substances contain, as is stated, much mercury, little sulphur, and less salt, then it is desirable to show that a vegetable may be constructed of a substance containing none of these principles, but only of water, which was then sometimes termed 'phlegm,' and was ranked among the elements. This he attempted by growing a 'pompion' in a weighed quantity of earth, and after the pumpkin had grown, he showed it to consist of water, by distilling it; and by weighing the earth, he proved that it had not lost weight. He then turns to the 'vulgar spagyrist,' and triumphantly challenges the truth of his theory. It is now known that the elements carbon and nitrogen, and

others in small quantity, must be added to those contained in water to produce a 'pompion'; but it was a great step to show that no salt, sulphur, or mercury were necessary. Boyle viewed the 'pompion' as simply transmuted water. He quotes from M. de Roche, who stated that he had transmuted earth into water, and *vice versâ.* Of the correctness of M. de Roche's opinion, he is not quite sure, but he attaches a certain amount of weight to it.

His second line of attack is to prove that the so-called elements are themselves further resolvable. And beginning with sulphur, he points out that what the chymists understand by sulphur has not always the same properties. It is, however, always inflammable. Sulphur, in the then accepted meaning of the word, was the inflammable portion obtained on distilling an animal or vegetable substance; mercury, another portion, not miscible with the sulphur; but uninflammable, and having taste; the residue on incineration, or, as it was termed, the *caput mortuum,* was salt. In an old writing on the subject, salt is said to be the basis of solidity and permanency in compound bodies; oil or sulphur (the two words came to have nearly the same meaning) serves the purpose of making the mass more tenacious; mercury is to leaven and to promote the ingredients, and earth is to soak and dry up the water in which the salt is dissolved.

We note here a change in the manner of regarding elements. They are no longer principles, or abstract qualities of matter, but they *exist* in the matter, and can be extracted from it by suitable processes. Their number varied; and phlegm or water was now accepted as elementary, now rejected, as suited the purpose of the theorist. Boyle clearly showed that these elements had not always the same properties; that the sulphur and mercury not only differed in every respect from brimstone and quicksilver, but that one variety, obtained by distilling wood,

differed from that obtained by submitting bones to the same process. He clinched his point by distilling the distillates themselves in turn—in fact by performing what we now call a 'fractional distillation'—and showed that it was possible to divide them in turn into several liquids differing from each other in properties. In this he anticipated a process now practised on a very large scale, namely the manufacture of vinegar from wood, which he successfully separated from wood-spirit and tar.

Almost all research, before Boyle's time, employed two processes, ignition or heating in contact with air, and distillation, or heating in a vessel of irregular shape, named an alembic, leading the vapours through a cooled tube still called a worm, and collecting the liquefied product in a pear-shaped vessel, named a receiver. Heat was assumed to be the universal resolver of bodies; and the products of the action of heat on compounds were accepted as elements. Boyle doubted this; he questioned whether the products obtained on distillation were pre-existent in the substances distilled, as the theory of elements would require. He found that on distillation the same substances are not always produced, nor the same number; and he demonstrated that these products themselves are not pure or elementary bodies, but 'mixts.' He says: 'It is to be doubted whether or no there be any determinate number of elements, or if you please, whether all compound bodies do consist of the same number of elementary principles or ingredients.'

But Boyle was not merely a destroyer; he also, if not in so orderly a manner, attempted to construct a theory of his own. He appears to have held the notion of a universal matter, and to have conceived the different varieties to be due, not to the presence of separable properties, but to the form and motion of its minute portions. In supporting this doctrine against the theories prevalent

in his time, he says: 'I demand also, from which of the chymical principles motion flows, which yet is an affection of matter much more general than can be deduced from any of the three chymical principles.' In an essay entitled 'The history of Fluidity and Firmness,' he endeavours with some success to show that all bodies, even those which appear most rigid, are in motion. For example, he points out that the diamond when rubbed shines in the dark, and in conformity with our present views, attributes that to molecular motion. He also notices that all bodies expand by heat, and is inclined to ascribe the magnetisation of steel to the motion of its minute particles. He attributes the varying properties of matter to motion and rest. In yet another passage, he supposes the action of acids on metals to be due to the pointed shape of their atoms, which, by inserting themselves between the more rounded particles of the metal, wedge them asunder, and themselves become blunt during the process.

It is difficult to overestimate the value of Boyle's labours in the field of chemistry. Although he was the first to proclaim that chemistry is independent of any art, and must be regarded as part of the great field of nature, yet the practical benefit which has accrued to mankind through Boyle's theoretical as well as his practical work is incalculable. It was not until after his time that it was possible to construct a theory explaining the rule-of-thumb methods of manufacture which were formerly employed, and to render improvement and discovery no longer a matter of chance, but of reasoning. The whole progress of modern manufacture due to the elaboration of scientific discoveries, themselves the result, not of haphazard trial, but of careful and systematic investigation, sufficiently attests the benefit conferred by him in the practical application of scientific principles.

Time would fail to tell of Boyle's well-known memoir

'Touching the Spring of the Air,' in which he describes experiments proving that a volume of air under a pressure of two pounds occupies exactly half the volume that it does under a pressure of one pound. This, although not absolutely true, is yet sufficiently exact to be generalised into a law, which is known by Boyle's name. He finds a reason for this 'spring' in premising that 'the air abounds in elastic particles, which being pressed together by their own weight constantly endeavour to expand and free themselves from that force; as wool, for example, resists the hand that squeezes it, and contracts its dimensions; but recovers them when the hand opens, and endeavours at it even while that is shut.'

In truth Boyle delighted in mechanical explanations. The titles of his papers attest this. We find, 'The Mechanical Production of Magnetism'; 'The Mechanical Production of Electricity'; 'The Mechanical Causes of Precipitation'; 'The Mechanical Origin of Corrosiveness and Corrosibility'; and even, 'The Mechanical Production of Tastes and Colours.' The series finishes with 'The Mechanical Origin of Heat and Cold.' To produce heat it is necessary 'that the moving particles should be small'; and 'agitation is requisite to heat';—in fact, a statement, in language of the time, of modern views. In accounting for the decomposition of bodies by heat, his words are: 'It rather seems that the true and genuine property of heat is to set amoving and thereby dissociate the particles of matter.'

In spite of Boyle's numerous attempts to account for natural phenomena in terms of matter and motion, his modesty led him to make this statement: 'Having met with many things of which I could give myself no probable cause, and some things to which several causes may be assigned, so differing as not to be able to agree in anything unless in their all being probable enough; I have often

found such difficulty in searching into the cause and manner of things, and I am so sensible of my own disability to surmount these difficulties, that I dare speak positively of very few things except of matters of fact.' This, I think, is in the main still our position.

Boyle's claim to rank as a 'Great London Chemist' rests upon his having taken up his residence here from the year 1668, until his death, which took place on the last day of the year 1691, in the sixty-fifth year of his age. But he was not a Londoner by birth. He was an Irishman, born at Lismore in County Waterford, and of noble parentage, for he was the seventh son, and the fourteenth child, of the Earl of Cork. He was educated as a child at home; but at the age of eight he was sent to Eton, where, as he says, 'he lost much of that Latin he had got; for he was so addicted to the more solid parts of knowledge, that he hated the study of bare words naturally.' At the age of eleven (they were precocious in those days) his career at Eton was over; and he was sent with a French tutor, along with his brother, to Geneva, where he pursued his studies for twenty-one months, and then went to Italy. There he stayed until 1642; when his father's finances having become embarrassed, owing to the breaking out of the great Irish rebellion, Boyle returned home, to find his father dead. Two estates had been left to him; one at Stalbridge, in Dorsetshire, where he proceeded to reside. In 1654, when twenty-seven years of age, he removed to Oxford, in order to associate himself with a number of men who had united themselves into a society, under the name of the 'Philosophical College.' This society afterwards moved its headquarters to London; and in 1663 it was incorporated by Charles II., under the name of the 'Royal Society of London,' its object being the 'Promotion of Natural Knowledge.'

Boyle's name is frequently mentioned in the first few

volumes of 'The Transactions.' Thus we find on January 2, 1601, that 'Mr. Boyle was requested to bring in his cylinder, and to show at his best convenience the experiment of the air'; but his convenience was long in arriving, for on March the 20th 'Mr. Boyle was requested to remember his experiment of the air,' and on April 1 'he was desired to hasten his intended alteration of his air-pump.' On May 15, 'Mr. Boyle presented the Society with his engine,' and with it numerous experiments were made in the presence of members of the Society. In such 'philosophical' pursuits he spent his uneventful life; and, to quote his own words, from a biographical sketch drawn up by himself at an advanced period of his life, he says: 'To be such parents' son, and not their eldest, was a happiness that our Philarethes [a lover of virtue—himself] would mention with great expressions of gratitude; his birth so suiting his inclinations and designs, that had he been permitted an election, his choice would scarce have altered God's discernment.'

Cavendish, like Boyle, was also of noble birth. He was the son of Lord Charles Cavendish, himself the third son of the second Duke of Devonshire. His mother was Lady Anne Grey, fourth daughter of Henry, Duke of Kent. But except in the fact of their both being of the higher rank of society, and in their both being addicted to the pursuit of science, they have little in common. Boyle's mind roamed over the whole domain of nature; his writings treat of religious, philosophical, and scientific subjects with a fulness and lack of mental reserve which testify to his frank, transparent character. His motto was *Nihil humanum a me alienum puto*; and he carried this motto into his life and work. Cavendish, on the other hand, was by nature very shy and reserved; he had no friends, and few acquaintances; and instead of discussing the whole of nature, as did Boyle, he limited himself to the

investigation of a few problems of first-rate importance. His work is characterised by the utmost accuracy and elegance; and he was cautious to an extreme in announcing his conclusions. Both types of mind have their good side; but in their case one might have wished for a little more moderation. Had Boyle not been so many-sided, he might have advanced science more by accurate experimental work; and had Cavendish not been so reserved, he would have done more good to his contemporaries, and he would certainly have been a happier man. Neither was married; and it is perhaps legitimate to draw the conclusion that man's nature does not culminate in its best without the influence of a helpmeet.

Like Boyle's, Henry Cavendish's life was an uneventful one, and may be told in a few words. He was born on the 10th October 1731, at Nice, where his mother had gone for her health. She died when he was two years old. In 1742, he became a pupil of Dr. Newcome, at Hackney School, where he stayed until 1749; in that year, he matriculated at Cambridge, and entered as a student at Peterhouse. In 1753, he left without taking his degree; he probably went to London; but all details of his life are lacking for the next ten years, though it is probable that he spent the major part of his time in mathematical and physical studies, and in research in the stables belonging to his father's town house, which he had fitted up as a laboratory. It was not until 1766 that he summoned up resolution enough to publish; although his note-books show that in 1764 he had begun to make experiments which would have been well worth recording. From that time forward, until 1809, the year before his death, his papers appeared in constant succession. There was little interruption to this incessant work, unless we consider a series of journeys made through various parts of England and Wales with the object of studying the geology of the

country, and the manufactures carried on in the variou industrial centres, as a species of holiday. There wa no weekly interruptions to his labours; Sunday as we as weekday was devoted to research, and so the year glided past. During his father's lifetime, he is said t have had an income of £500 a year; but at his father' death in 1783, and afterwards, owing to the legacy of a aunt, he became possessed of enormous riches. Indeed M. Biot, in pronouncing a biographical oration on Caven dish, used the phrase: '*Il était le plus riche de tous le savants, et probablement aussi, le plus savant de tous le riches.*'

His town house was at the corner of Montague Plac and Gower Street; visitors, however, were rarely ad mitted; and Cavendish kept his library for his own us and for that of the scientific public in a separate house i Dean Street, Soho. To this library he went for his own books, signing a formal receipt, as one would do at a public library, for each one borrowed.

His laboratory was a villa at Clapham. The uppe rooms were an astronomical observatory. Here h occasionally entertained friends, but in an unostentatiou way. His standing dish was a leg of mutton. It i related that on one occasion, when the unprecedente number of five guests had been invited, his housekeepe ventured to point out that one leg of mutton would b insufficient fare for so many; his answer was, 'Well, then get two.' Several of his contemporaries have left a record of their personal impressions of him. Professo Playfair described him as of an awkward appearance without the look of a man of rank. He spoke ver seldom, and then with great difficulty and hesitation, bu exceedingly to the purpose, his remarks either displayin some excellent information, or drawing some importan conclusion. An Austrian gentleman to whom he had

been introduced, after the fashion of his country, assured him that his principal reason for coming to London was to see and converse with one of the greatest ornaments of his age, and one of the most illustrious philosophers that ever existed. To all these high-flown speeches Mr. Cavendish answered not a word, but stood with his eyes cast down, quite abashed and confounded. At last, spying an opening in the crowd, he darted through it with all the speed he could muster, nor did he stop until he reached his carriage, which drove him directly home. Sir Humphry Davy said of him: 'His voice was squeaking, his manner nervous; he was afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty. He wore the costume of our grandfathers; was enormously rich, but made no use of his wealth.' And Lord Brougham's recollection was that he would often leave the place where he was addressed, and leave it abruptly, with a kind of cry or ejaculation, as if scared and disturbed. 'I recollect,' said Lord Brougham, 'the shrill cry he uttered, as he shuffled quickly from room to room, seeming to be annoyed if looked at, but sometimes approaching to hear what was passing among others.'

On occasion, he was not ungenerous, although the thought of giving did not occur to him. When dining one evening at the Royal Society Club, some one present mentioned the name of a gentleman who had previously acted as a temporary librarian in his library. Mr. Cavendish said, 'Ah! poor fellow, how does he do? How does he get on?' 'I fear very indifferently,' said this person. 'I am sorry for it,' said Mr. Cavendish. 'We had hopes that you would have done something for him, sir.' 'Me, me, me, what could I do?' 'A little annuity for his life; he is not in the best of health.' 'Well, well, well, a cheque for £10,000, would that do?' 'O sir, more than sufficient, more than sufficient.'

Solitary he lived, and solitary was his death. Having been ill for several days, his valet was called to his bedside, and told to summon Lord George Cavendish, as soon as he should be dead. In about half an hour he again summoned the servant, and made him repeat the message. He then said, 'Right. Give me the lavender water. Go.' Half an hour later the servant returned to his room, and found that he had expired.

If Boyle found interest in all things human, Cavendish appeared to take no thought of anything, except phenomena. As his biographer, Dr. George Wilson, said, his motto was *Panta metro, kai arithmo, kai stathmo* (*Πάντα μέτρῳ, καὶ ἀριθμῷ, καὶ σταθμῷ*). This we shall now learn, from a short consideration of his work.

Cavendish's earlier work is only to be found in his unpublished papers. It appeared to have been his habit, for some time, to write an account of his experiments, without any intention of bringing them to the notice of the public. An account of two long investigations was found among his papers, after his death, of a date considerably prior to that on which his first memoir appeared in the *Philosophical Transactions*. The first of these deals with the differences between 'regulus of arsenic' (metallic arsenic) and its two oxides. He concluded that arsenic oxide was 'more thoroughly deprived of its phlogiston' (in modern language, more thoroughly oxidised) than arsenious oxide; and the latter, than arsenic itself. The paper also contains speculations on the nature of the red fumes obtained in the conversion of arsenious to arsenic oxide by means of nitric acid; speculations which were afterwards to bear rich fruit, in his work on the composition of air.

Another of his unpublished researches deals with heat. Cavendish discovered independently the laws of specific heat; and he collected tables of the specific heats of many

substances. He also was acquainted with what Black termed 'latent heat,' that is, the heat absorbed during the evaporation of liquids, or which is evolved during the condensation of gases or vapours, or the solidification of liquids.

As this essay deals with Cavendish as a chemist, I shall treat very shortly of his physical work. One of the most important of his investigations has reference to the cause of the shock given by that curious fish the torpedo. By constructing a species of artificial torpedo, he proved that the shock was due to an electric discharge; and what is more, he was the first to distinguish between electric quantity and electric intensity. Indeed, these terms are due to him, as Faraday has acknowledged.

In 1783, 1786, and 1788, he published three papers on freezing, in which his views on the nature of heat were expounded. The first of these deals with the freezing of mercury; the second and third, with the congelation of the mineral acids, and of alcohol. He objected to Black's expression, 'the evolution, or setting free of latent heat,' as involving an hypothesis that the heat of bodies is owing to their containing more or less of a substance called the matter of heat. He preferred to adopt Boyle's and Sir Isaac Newton's supposition that heat consists in the internal motion of the particles of bodies. And he therefore uses the expression 'heat is generated.'

An interesting part of the last of these papers is a passage in which he anticipates Richter's tables of the equivalents of the acids and bases, not by any elaborate disquisition, but as a device for estimating the strength of sulphuric acid. In 1788 he wrote: 'The method I used was to find the weight of the *plumbum vitriolatum* formed by the addition of sugar of lead, and from thence to compute the strength, on the supposition that a quantity of oil of vitriol, sufficient to produce 100 parts of *plumbum*

vitriolatum, will dissolve 33 of marble; as I found by experiment that so much oil of vitriol would saturate as much fixed alkali as a quantity of nitrous acid sufficient to dissolve 33 of marble.' Richter's tables were published in 1792. Cavendish's remarks involve a knowledge of fixity of proportion, and also of reciprocal proportions; doctrines which were after nearly twenty years propounded by Dalton.

Perhaps the most important piece of physical work ever performed was Cavendish's determination of the constant of gravitation, or as it is often called, 'the weight of the earth.' The experiment is usually spoken of as the 'Cavendish experiment,' although the method of executing it was first suggested by the Rev. John Mitchell. A delicate torsion balance, suspended by a wire, had leaden balls suspended at each end. Two heavy spherical masses of metal were brought near the balls, so that their attraction tended to draw the two balls aside. The deviation of the arms was observed, or calculated from the time of vibration; and from the data found, it is easy to calculate the attraction of a sphere of water, equal in mass to the ball or a similar ball resting on its surface; and so to determine the density of the earth, knowing the attraction which it exerts on the ball. The results obtained compared very favourably with the best results obtained by other observers, using the utmost precautions; and it is a very remarkable instance of Cavendish's experimental skill and ingenuity.

We have here to consider more particularly Cavendish's chemical work. It was of the highest order, and bears the imprint of a master mind, guiding a master hand.

Before Black's time, the word 'gas' had no plural. Indeed, what we now know as a gas was set down as a modification of ordinary air. Black, however, proved that

a gas could be contained in a solid state, as for instance in carbonate of lime or of magnesia, or in what were then known as the 'mild alkalies'; and that it could possess weight. He termed carbonic anhydride 'fixed air.' Cavendish's first published paper deals with 'Factitious Air'; it appeared in 1766, seven years after the publication of Black's memoir on 'Magnesia alba, Quick-lime, and other Alkaline Substances.' 'Factitious air' was defined by Cavendish as 'any kind of air which is contained in other bodies in an unelastic state, and is produced from thence by art.' He first treats of hydrogen, next of carbon dioxide, and lastly of gases evolved during fermentation and putrefaction. Although not the first to prepare hydrogen (for it must have been known for centuries that an inflammable gas was evolved on bringing metals into contact with certain dilute acids), yet he was the first to characterise hydrogen as a definite substance, and not a mere variety of common air. He prepared this gas from zinc, iron, or tin, and weak sulphuric or hydrochloric acid. He found that the substance was identical in each case, by weighing a known volume; which he did with no great accuracy in a bladder, but with considerable exactitude by weighing a flask containing, for example, zinc and acid, unmixed; and after mixture, weighing again; a further experiment served to determine the volume of gas obtainable from a known weight of zinc. Another method of establishing their identity, curious to our notions, was to mix the sample with a known volume of air, and estimate the loudness of the explosion which took place on applying a flame. Cavendish also prepared 'the volatile sulphurous acid,' by substituting concentrated sulphuric acid for dilute; and a non-inflammable air (nitric oxide), by the action of nitric acid.

Cavendish did not suppose that the 'air' came from the acid, but from the metal. It must be remembered

that at that time, the current doctrine was that when substances burn, they lost a principle, to which the name 'phlogiston' had been applied by Stahl, the propounder of the doctrine. The hydrogen evolved was at first supposed by Cavendish to be the long-sought phlogiston itself. But fuller consideration induced him to change his view; and he subsequently held that hydrogen was a hydrate of phlogiston, or a compound of that hypothetical substance with water. In this paper, too, as well as in one which followed, Cavendish added many facts to those which had been published by Black on the properties of carbonic acid; but as these contain little of theoretical interest, they need not detain us.

Seventeen years later, the next of his 'pneumatic' papers was published. It was entitled, 'An Account of a New Eudiometer.' The eudiometer, which in no way resembled the picture of the instrument usually ascribed to him, was designed, not for the explosion of a mixture of two gases, but for the removal of oxygen from air, by means of nitric oxide. With its aid, he determined the composition of many samples of air, and his final result, translated into our method of statement, gave for the proportion of oxygen in air the extraordinarily accurate number, 20·83 per cent.

Cavendish's next paper, in order of publication (1784) gave the results of experiments begun in 1781. Its title is 'Experiments on Air.' The object of these experiments was to 'find out the cause of the diminution which common air is well known to suffer by all the various ways in which it is phlogisticated, and to discover what becomes of the air thus lost or condensed.' His first idea was that this treatment might result in the formation of 'fixed air.' But having disproved this, he proceeded to try whether, as some of Priestley's experiments appeared to show, 'the dephlogisticated part of common air might

not by phlogistication be changed into nitrous or vitriolic acid'; *i.e.* whether oxygen, by reduction, might not be converted into nitric or sulphuric acid. Absorbing the oxygen by burning sulphur, he failed to find nitric acid; and using nitric oxide as the absorbent, the resulting nitrate and nitrite contained no sulphate. He therefore tried firing a mixture of hydrogen and air by means of an electric spark; an experiment which led to the discovery of the composition of water. Having burned 500,000 grain measures of inflammable air (hydrogen) with two and a half times its volume of common air, he collected upwards of 135 grains of water, 'which had no taste nor smell, and which left no sensible sediment when evaporated to dryness.'

It is impossible in a short sketch like the present to enter into a description of the exceedingly ingenious experiments devised to show whence the acid was derived which is formed when the hydrogen is present in insufficient amount; we must be content to remember that in default of hydrogen with which to combine, some of the oxygen unites with the nitrogen, yielding nitrous and nitric acids.

Although Cavendish employs the language of the phlogistic theory in stating his conclusions, yet it must not be supposed that he was ignorant of the newer views, propounded by Lavoisier. In the memoir which we have been considering, he states his conclusions in the new phraseology; but he concludes as follows: 'It seems, therefore, from what has been said, as if the phenomena of nature might be explained very well on this principle without the help of phlogiston; and indeed, as adding dephlogisticated air to a body comes to the same thing as depriving it of its phlogiston, and adding water to it, and as there are perhaps no bodies entirely destitute of water, and as I know no way by which phlogiston can be

transferred from one body to another without leaving it uncertain whether water is not at the same time transferred, it will be very difficult to determine by experiment which of these opinions is the truest; but as the commonly received principle of phlogiston explains all phenomena, at least as well as Mr. Lavoisier's, I have adhered to that.' We shall meet with this same difficulty again, when we consider Davy's experiments, which led to true views concerning the nature of chlorine.

Cavendish's aim in these experiments, stated in modern language, was to find out what becomes of the oxygen, when substances burn in air; whether the production of carbon dioxide is a constant accompaniment of combustion. He mentions five ways in which air may be deprived of oxygen, namely, by the calcination of metals; by burning in it sulphur or phosphorus; by mixing it with nitric oxide; by exploding it with hydrogen; and lastly by submitting it to the action of electric sparks. In the second series of his experiments on air, he examines in detail the action of a continued rain of sparks on air; and this led to the discovery of the composition of nitric acid; for the 'caustic lees' on evaporation to dryness 'left a small quantity of salt, which was evidently nitre, as appeared by the manner in which paper impregnated with a solution of it burned.' But he doubted whether 'there are not, in reality, many different substances confounded by us under the name of phlogisticated air.' He 'therefore made an experiment to determine whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change.' On experiment, he found that 'if there is any part of the phlogisticated air of our atmosphere which differs from the rest, and cannot be reduced to nitrous acid, we may

safely conclude that it is not more than $\frac{1}{125}$ part of t
whole.' Here he was nearly right; about one per ce
is actually left; and it has been recently recognised as
separate element, and named *Argon*. And still m
recently, the argon has been shown to contain a sm
proportion of other gases, also elements, to which the nan
helium, neon, krypton and xenon have been given. T
paper was the last on chemical subjects published
Cavendish.

These two men, Boyle and Cavendish, both rank
great men. The first has been termed with justice 't
father of modern chemistry'; the second by 'weighing t
earth,' and by establishing the composition of water a
of air, has even more decided claims to that title. Ea
was in advance of his age: Boyle by reason of his ca
philosophical spirit, and clear judgment; Cavendish
the power he possessed, in an age of qualitative e
deavours, of carrying out quantitative experiments w
the most refined accuracy, and of drawing from th
correct conclusions.

II. DAVY AND GRAHAM

Between a prospect over an extensive landscape, and
retrospect in history, an instructive analogy may
drawn. It is true that when the spectator is remov
from the object by a great distance, whether of time
space, its appearance is ill-defined and hazy, as are to
the personalities of the ancient Egyptians, Greeks, a
Arabians; and just as the imagination supplies details
the distant features of a landscape, details which may
may not be in consonance with fact, so through the mi
of time we are apt to read into the writings of t

ancients ideas which have their origin rather in our own brains than in their works. Objects in the middle distance are perhaps most truthfully interpreted. They are not obscured by the haze of perspective nor by the multitudinous aggregations of propinquity. So it is with Boyle and with Cavendish. But with Davy, and with Graham, whose lives and works are to form the subject of this essay, it is difficult to select from their writings those salient features which will, in the course of another half-century, stand out clearly and luminously among the labours of their contemporaries. In chemical and physical work, as in life, safety lies in a happy mean; and it shall be my endeavour to avoid unimportant details, while presenting the main characteristics of the work of these two remarkable men. The difficulty is to know what to omit; for that which appears unimportant to-day may to-morrow turn out to be essential to the fundamental doctrines of our science.

At the time when Cavendish was beginning his splendid series of experiments on gases, Humphry Davy, an infant of two, was beginning to show signs of that ability which so remarkably distinguished him in after life. At that age, he could speak fluently; a year or two later, he was sent to school, where he learned to read and write before he was six; and in his seventh year he was sent to the Grammar School at Truro, his native place. Looking back on his experiences there, from the standpoint of a young man of twenty-two, he wrote: 'I consider it fortunate that I was left much to myself when a child, and put upon no particular plan of study, and that I enjoyed much idleness at Mr. Coryton's school.' Do not we err in insisting too much on the systematic employment of time by the boys of our modern schools? For, be it remembered, the compulsory cricket and football, so common in our schools, is to some boys the hardest task

they have to master, and leaves no time for salutary idleness.

Like many boys, Davy entered the study of chemistry through the doorway of fireworks. His favourite amusements were fishing, and the art of rhyming. During his whole life, he never lost the taste for these two pursuits; and though it must be confessed that he was a more successful fisher than poet, still his verses have a certain amount of merit, and betoken a considerable gift of imagination, necessary to the higher achievements in science, as he indicates in the two stanzas which I venture to quote:—

> While superstition rules the vulgar soul,
> Forbids the energies of man to rise,
> Raised far above her low, her mean control,
> Aspiring genius seeks her native skies.
>
> She loves the silent, solitary hours;
> She loves the stillness of the starry night,
> When o'er the bright'ning view Selene pours
> The soft effulgence of her pensive light.

In his later efforts he preferred decasyllabics; and though his sentiments thus expressed are praiseworthy, his execution rarely exceeds the level demanded from a poet laureate.

At the early age of fifteen, his school education was at an end. For the next year he continued in the 'enjoyment of much idleness.' But in the beginning of the year 1795 he was apprenticed to Mr. Borlase, surgeon and apothecary, in his native town. Then the demon of work seized on him, and he threw himself into the task of self-improvement with irresistible ardour. His scheme of study is so remarkable, and so extensive, that I cannot

resist the temptation to quote it at full length. Here it is :—

1. Theology or Religion, taught by Nature.
   Ethics, or moral virtues, by Revelation.
2. Geography.
3. My Profession—
   1. Botany.
   2. Pharmacy.
   3. Nosology.
   4. Anatomy.
   5. Surgery.
   6. Chemistry.
4. Language—
   1. English.
   2. French.
   3. Latin.
   4. Greek.
   5. Italian.
   6. Spanish.
   7. Hebrew.
5. Logic.
6. Physics.
   1. The doctrines and properties of natural bodies.
   2. Of the operations of nature.
   3. Of the doctrines of fluids.
   4. Of the properties of organised matter.
   5. Of the organisation of matter.
   6. Simple astronomy.
7. Mechanics.
8. History and Chronology.
9. Rhetoric and Oratory.
10. Mathematics.

Which of us has undertaken a course of study so extensive, and so inclusive?

Following out this course, not quite in the prescribed order, however, he reached the subject of chemistry in January 1798. His textbooks were Lavoisier's *Chemistry* and Nicholson's *Dictionary of Chemistry*. He kept up the study of mathematics during the whole course, having begun in 1796: for he remarks on its usefulness as a preliminary to the study of chemistry and physics. In his self-imposed task of mastering chemistry, he at once began practical work, having fitted up a small laboratory, furnished with the very simplest and most inexpensive

apparatus, in Mr. Tonkins's house. About four months after beginning his chemical studies he was in correspondence with Dr. Beddoes, a medical man residing at Clifton, on the subject of heat and light. This correspondence was fraught with momentous consequences for Davy; for it led to his being offered the position of superintendent of the 'Pneumatic Institution,' founded by the doctor, with the help of Josiah Wedgwood and Mr. Gregory Watt, youngest son of James Watt, with the object of experimenting with the gases, at that time recently discovered, in order to ascertain whether they would prove suitable as remedial agents.

In reviewing the career of a man, it is interesting to note the motives which underlie his actions. The latter, indeed, may not always be worthy of the sentiments which give them birth, but it is just to give credit for pure intentions, and to form an estimate of character by taking both motive and action into consideration. In one of the earliest of Davy's notebooks, intended for no eye but his own, there is this entry: 'I have neither riches, nor power, nor birth to recommend me; yet, if I live, I trust I shall not be of less service to mankind and to my friends than if I had been born with these advantages.' And again, in 1821, nearly twenty-five years later, his diary contains the aspiration, 'May every year make me better—more useful, less selfish, and more devoted to the cause of humanity and science.' These are noble words, and they lead one to form a high estimate of the character of Humphry Davy.

In January 1799 he went to the Pneumatic Institute, and worked under the patronage of Dr. Beddoes. By the following year he had finished his classical research on nitrous oxide, and had discovered and investigated its remarkable anæsthetic properties. He also discovered the composition of nitric acid, nitric oxide, nitric peroxide,

and ammonia. By 1801 he had begun his experiments with the 'galvanic battery,' which was to be so fruitful of important results in his hands. During these two years, he published no fewer than nine papers in the scientific journal of his time, *Nicholson's Journal*, the predecessor of the *Philosophical Magazine*,—the result of astonishing industry.

At this period of his life, Davy's acumen led him to avoid undue theorising, and to endeavour to accumulate facts. His own words are: 'When I consider the variety of theories that may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's.' In the light of this opinion, it is interesting to examine the programme which he laid down for himself at the time. It was written in the spring of 1799, and is as follows:—

'To decompose the muriatic, boracic, and fluoric acids; to try triple affinities, and the contact with heated combustible bodies at a high temperature.

'To ascertain all the phenomena of oxydation.

'To discover with accuracy the vegetable process.'

The decomposition of the muriatic and the boracic acids was successfully accomplished at a much later date. But the 'phenomena of oxydation' are even now known only imperfectly. He contributed useful facts, however, as we shall see, to our knowledge of 'the vegetable process.'

Consistently with these ideas regarding the relative merits of theory and practice, Davy made his greatest successes in the realm of facts. Where he attempts theorising, the results are not happy. It is true that he did not risk the publication of his theories; but those

revealed by his notebooks have not much to recommend them. He allowed his imagination, of which he possessed a rich share, full scope in other directions. Many of his imaginative projects were, however, not realised. Among them may be mentioned an epic poem, in six books, entitled *The Epic of Moses*, written, what there is of it, in decasyllabics. He possessed a deeply religious nature; and he regarded 'this little earth as but the point from which we start towards a perfection bounded only by infinity.'

In 1801 Davy was recommended by Professor Hope of Edinburgh for the lectureship at the Royal Institution, which had been founded a few years previously by Count Rumford, on the resignation of Dr. Garnet, the first Professor of Chemistry there. He delivered his first lecture in April 1801, and he at once achieved a great success. To quote from an account by a contemporary witness: 'The sensation created by his first course of lectures at the institution, and the enthusiastic admiration which they obtained, is at this period hardly to be imagined. Men of the first rank and talent—the literary and the scientific, the practical and the theoretical—bluestockings and women of fashion, the old and the young, all crowded, eagerly crowded, the lecture-room. His youth, his simplicity, his natural eloquence, his chemical knowledge, his happy illustrations and well-conducted experiments, excited universal attention and unbounded applause. Compliments, invitations, and presents were showered on him in abundance from all quarters; his society was courted by all, and all appeared proud of his acquaintance.' With all these temptations to neglect his work, he remained faithful to his charge. In 1803 he wrote: 'My real, my *waking* existence is among the objects of scientific research. Common amusements and enjoyments are necessary to me only as dreams to interrupt the flow of

thoughts too nearly analogous to enlighten and vivify.' Still many of our scientific workers of to-day would be glad if they could extract as much leisure time from amidst their daily employments. Davy generally entered the laboratory about ten or eleven o'clock, and if uninterrupted, remained there till about three or four. His evenings were almost invariably spent in dining out, and at evening parties afterwards. 'To the frequenters of these parties he must have appeared a votary of fashion, rather than of science,' as his brother remarked.

Yet, during the years which followed, he accomplished an immense amount of very remarkable work. Besides investigating, by the request of the managers of the Royal Institution, the chemistry of tanning, an investigation which led to the use of catechu as a substitute for the old-fashioned oak-bark, he lectured, by the request of the Board of Agriculture, on 'The Connection of Chemistry with Vegetable Physiology.' These lectures were given every year, and in them were incorporated the results of a considerable number of experiments made by him, or under his direction, on the chemistry of plants. In 1813, when he ceased to lecture on the subject, he published his lectures, under the title *The Elements of Agricultural Chemistry*. For the copyright of this work he received one thousand guineas, and fifty guineas for each subsequent edition. Truly he was a fortunate man!

Between January 1801 and April 1812 he accomplished two of his most remarkable pieces of work; first, on the decomposition of the alkalies; and second, on the nature of chlorine. As his name lives chiefly in connection with these two investigations, and in his research on the nature of flame, which culminated in the invention of the safety-lamp, I shall give some account of them in minuter detail.

The Swedish chemist, Scheele, had discovered in 1774,

that on treating manganese dioxide with hydrochloric acid, or as it was then called 'spiritus salis,' in a flask to which a bladder had been attached, a 'yellow air' filled the bladder, which possessed a suffocating smell, which bleached litmus paper and flowers, and which attacked metals, even gold. He named this new gas 'dephlogisticated marine acid,' imagining that the manganese had deprived the marine acid of its 'phlogiston,' and that it had consequently been converted into the yellow gas. Count Berthollet, in 1788, prepared this gas, and on saturating with it water cooled with ice, he discovered that a solid crystalline hydrate separated from the water. Having exposed a solution, thus obtained, to sunlight, he noticed the evolution of oxygen, and he, therefore, concluded that the dephlogisticated marine acid was in reality a compound of marine acid with oxygen, since, under the action of sunlight, oxygen was evolved, and marine acid left. This idea, according to Berthollet, readily explained the action of the solution of the yellow gas on metals; for it might be supposed to give up to metals its oxygen, and the metallic oxide would then, as usual, dissolve in the marine acid. In consequence of this observation, M. de Morveau, in conjunction with Lavoisier, Berthollet, and de Fourcroy, in drawing up their *Méthode de nomenclature chimique*, proposed for the gas the name 'Oxymuriatic acid.' To follow the further history of chlorine, it will be advisable to pause, and consider Davy's researches on the alkali metals.

Before leaving Bristol, Davy had begun experiments with the galvanic battery. On reaching London, he continued his electrical work; and in 1807 he published a remarkable paper on the 'Chemical Agencies of Electricity.' It had been shown that when the two poles of a battery with platinum terminals were plunged into two vessels of water, connected together by wet asbestos, or

cotton wick, an acid appeared round the positive wire, and an alkali round the negative wire. Davy showed by a series of convincing experiments that the alkali is usually potash or soda derived from the glass, and the acid usually hydrochloric acid from the common salt present as an impurity in the water. From experiments such as these he evolved a theory that all substances which have a chemical affinity for each other are in opposite states of electrification, and that the positive pole attracts those constituents of the solution which possess a negative charge, while the negative pole attracts the positively charged component. The more powerful the battery, the greater the force of these attractions and repulsions. For example, oxygen and acids are negative bodies, for they are attracted by the positive pole, and liberated there; whereas metals and their oxides, and hydrogen, nitrogen, carbon, and selenium are positive, because they separate at the negative pole. It ought, therefore, to be possible, by help of a sufficiently strong electric current, to decompose any compound whatsoever. Davy carried his inference farther, and suggested that the reason of chemical attraction is the oppositely charged state of the components of a compound. A compound is an electrically neutral body, for the constituents of the compound, in uniting, have respectively equal and opposite charges, which neutralise each other by the act of combination. But a current of electricity, passing through such a compound, might neutralise the electricity in each, and so, by overcoming their attractions, decompose the compound.

By applying these ideas, he succeeded in decomposing the 'fixed alkalies,' as caustic soda and potash used to be called, into oxygen, hydrogen, and the metals sodium and potassium. Having failed to obtain any products from aqueous solutions of these compounds, except oxygen and

hydrogen, he next attempted to pass a very powerful current through the fused alkalies. Potash was fused in a platinum spoon, connected with the positive side of a battery; and a platinum wire, connected with the negative pole of the battery, was dipped into the fused alkali. The result was an intense light at the negative wire, and a column of flame from the point of contact. On reversing the current, 'aeriform globules, which inflamed in the atmosphere, rose through the potash.' The substance produced was evidently inflammable, and was destroyed at the moment of liberation. Better results were obtained by the use of slightly moist potash; and small metallic globules were collected, 'precisely similar in visible characteristics to quicksilver.' 'These globules numerous experiments soon showed to be the substance I was in search of, and a peculiar inflammable substance, the basis of potash.' Soda gave an analogous result; and thus the metals of the alkalies were discovered.

These new metals burned in oxygen, forming the alkalies from which they had been obtained; they also burned in 'oxymuriatic acid,' forming 'muriates' of potash or soda. They decompose water with evolution of hydrogen, giving solutions of the respective alkalies; and they form compounds with sulphur and with phosphorus. They reduce metals such as copper, iron, lead, and tin from their oxides; and they attack glass, apparently liberating the 'basis of the silex.'

Fairly accurate estimations were made of the proportion of these new metals in the alkalies, which were believed by Davy to be oxides; and thus the approximate composition of these compounds, which at one time were believed to be elements, was definitely established.

Although similar phenomena were seen with the alkaline earths 'barytes' and 'strontites,' it was not found possible to isolate the metals; but on electrolysing with a

negative pole of mercury, amalgams were obtained, containing the new metals barium and strontium; while from lime and magnesia, evidence was similarly obtained that they consisted of metals, named by Davy calcium and 'magnium.' On removing the mercury by distillation, white metallic residues were obtained, still containing mercury, but oxidising rapidly in the air, to the respective oxides. An account of these results was published in 1807 and 1808, in the *Philosophical Transactions.*

In December 1808, the celebrated paper on the elementary nature of chlorine was read. Having failed to obtain any other products than hydrogen and oxygen on passing a current through an aqueous solution of muriatic acid gas in water (why, is not so apparent, unless only dilute solutions had been employed), Davy treated dry muriatic acid gas with potassium. The gas was absorbed, yielding $\frac{8}{22}$ of its volume of hydrogen. He concluded from this that dry muriatic acid gas contained at least one-third of its weight of water, and that it had not been 'decompounded' by the potassium. His first attempt, therefore, was directed to obtaining really dry muriatic gas. For this object, he heated dry muriate of lime with dry sulphate of iron, with phosphoric glass, and with dry boracic acid; but without any evolution of gas, although when water was added to the ignited mass, quantities of muriatic gas were liberated. After numerous attempts of the same kind, during which the chlorides of sulphur and phosphorus were discovered, these substances were themselves submitted to the action of potassium, but without the formation of any gaseous product.

In an appendix to these observations, which were published as the Bakerian Lecture, Davy announces the view that 'muriatic acid gas is a compound of a substance, which as yet has never been procured in an uncombined state, and from one-third to one-fourth of water, and that

oxymuriatic acid is composed of the same substance (free from water), united to oxygen.' His idea then was that 'when bodies are oxidated in muriatic acid gas, it is by a decomposition of the water contained in that substance, and when they are oxidated in oxymuriatic acid, it is by combination with the oxygen in that body.' Davy believed that the chlorides all contained oxygen.

In a later paper, read in November 1809, he arrived at the true explanation of these facts. It was based on experiments on the ignition of charcoal to whiteness in muriatic and oxymuriatic gases. No action occurred; and Davy began to doubt whether, as universally supposed, these bodies contain any oxygen. He therefore tried whether compounds produced by the action of oxymuriatic acid on tin, phosphorus, and sulphur would give with ammonia precipitates of the oxides of these elements, or any compounds containing oxygen; and his experiments were attended with negative results. He next considered one argument that the so-called 'oxymuriatic acid' contained oxygen, viz. the fact that on treatment with metals, hydrogen is evolved; and in a further paper, read in November 1810, he proved that on heating barium or strontium in the gas, one volume of oxygen is liberated for every two volumes of oxymuriatic acid absorbed. This is exactly the amount of oxygen contained in the oxide; and experiments with other oxides of metals resulted in similar liberation of all the oxygen previously combined with the metal. From these facts, Davy concluded that 'to call a body which is not known to contain oxygen, and which cannot contain muriatic acid, oxymuriatic acid, is contrary to the principles of that nomenclature in which it is adopted'; and he therefore proposed for the gas the name chlorine.

Many derivatives of chlorine were made by Davy for the first time; among them were the oxygen compounds

of chlorine. But he did not commit himself to the dogmatic assertion that this gas is an element; on the contrary, he writes: 'In the views that I have ventured to develop, neither oxygen, chlorine, nor fluorine are asserted to be elements; it is only asserted that, as yet, they have not been decomposed.' It would be well, were all chemists to imitate Davy's caution.

These views were combated by Gay-Lussac and Thénard; but it would take too much time to follow the contest. Suffice it to say, that Davy came off with flying colours.

During all these years, honours were being showered on Davy. In 1803, he was made a Fellow of the Royal Society; in 1807, he was chosen for its secretary, an office which he held until 1812; and in the latter year he was knighted. In his private diary, in which he transcribed his inmost thoughts, there is a pleasant little sentence, recording sentiments on the subject of honours: 'A man should be proud of honours, not vain of them.' But besides honours, wealth was also his portion; for two courses of lectures in Dublin, he was paid no less a sum than £1170!

In 1812, his *Elements of Chemistry* was published. It was dedicated to his wife; for in that year he married Mrs. Apreece.

In the same year, he nearly lost his sight by experimenting with chloride of nitrogen, which had recently deprived its discoverer, Dulong, of a finger. In 1813, he established the true nature of fluorine, and demonstrated its analogy with chlorine; and towards the end of the same year, he paid a visit to Paris, conveying with him a portable laboratory, by help of which he proved the similarity of iodine to chlorine. That element, which had been discovered about two years previously by Courtois, was supposed by Gay-Lussac to yield an acid identical

with hydrochloric acid. Davy communicated his discovery to Gay-Lussac, who by no means agreed with his conclusions; and it was not until a considerable time had elapsed, and the latter chemist had carried out his masterly researches on iodine and its compounds, that he became convinced of the correctness of Davy's views.

On his return from this Continental tour, he devoted his time to the investigation of the nature of flame, with the result that he discovered how to prevent flame from spreading into the adjoining atmosphere, by surrounding it with a sheath of wire-gauze; the conducting power of the gauze so cooling the explosive mixture of gases, that they no longer inflame after traversing the gauze diaphragm. This invention was hailed with the greatest satisfaction by the public, as well as by those whose interest was bound up in mines; and in 1817, he was presented with a service of plate, valued at £2500, by the owners of many important collieries. His services to humanity were, indeed, valued so highly, that in the following year a baronetcy was bestowed on him. And in 1820, on the death of Sir Joseph Banks, who had presided over the meetings of the Royal Society for no less than forty-one years, Sir Humphry Davy received the highest honour which can be bestowed on a scientific man, in being elected his successor. He resigned the presidency in 1827. His own view regarding honours was: 'It is not that honours are worth having, but it is painful not to have them'; and again, 'It is better to deserve honours and not to have them, than to have them and not deserve them.' These sentiments remind one of Burns's rhyming grace before meat:

> Some hae meat, and canna eat,
> And some wad eat that want it;
> But we hae meat, and we can eat,
> And sae the Lord be thankit.

During these years, Davy published many papers, havin relation to the preservation of metals by electro-chemic means, with special reference to the preservation of th copper sheathing of ships. In 1826, these, and othe similar inquiries, were summed up in the 'Bakerian Lecture, on the Relation of Electrical and Chemic Changes.

His scientific work, however, was nearly at an end; fo in 1826 he had a slight shock of paralysis, and though h lived until 1829, it was in a continual search for healtl He travelled much on the Continent, and made partia recoveries; but he was seized by a final stroke at Genev in May 1829, where he died, in his fifty-first year.

Sir Humphry Davy's work is well summed up in notice published in Silliman's *American Journal o Science and Arts*: 'To conclude, we look upon Si Humphry Davy as having afforded a striking example o what the Romans called *a man of good fortune*;—whos success, even in their view, was not however the result o accident, but of ingenuity and wisdom to devise plans and of skill and industry to bring them to a successfu issue. He was fortunate in his theories, fortunate in hi discoveries, and fortunate in living in an age sufficientl enlightened to appreciate his merits.' But let him speak his own epitaph; it is: 'My sole object has been to serve the cause of humanity; and if I have succeeded, I am amply rewarded in the gratifying reflection of having done so.'

Fortunately for your patience, my task to-day is limited to sketching the lives of those chemists who have gone from among us. And confining myself to the names of those who must pass without cavil as 'great,' that of Graham presents itself. There have been men of considerable ability, who have in their day done good and

useful work; such men as Turner, Graham's predecessor; Daniel, who gave us the battery known by his name; Miller, to whose painstaking labours we owe the revision of our standards of weight and measure; and many others of less eminence. But of these I can only mention the names.

The city of Glasgow gave Graham to London; Boyle was an Irishman; Cavendish was born in France; and Davy came from Cornwall. But London made some return for depriving Glasgow of Graham; for Penny was a Londoner, who passed the major part of his life in Glasgow, having been called thither as successor to Graham. He, too, did good work in his day; he was an extremely attractive lecturer, and may be said to have brought the art of giving professional evidence to perfection. In the eyes of many, this last may prove no recommendation; but if it be regarded as unworthy of the character of a true man of science, voluntarily to abandon that most precious heritage of a genuine philosopher, an open mind, Penny atoned for his sins by many beautiful investigations, the most important of which are perhaps his determinations of atomic weights, determinations which even to-day rank among the most reliable.

Thomas Graham was the son of a Glasgow manufacturer, and was born towards the end of the year 1805. He was educated in the Glasgow High School, and afterwards at the university there. His university career lasted an unusually long time; for entering when he was fourteen years of age, he did not graduate until he had reached the mature age of twenty-one. I am well aware that to an Oxford or Cambridge 'man,' the age of fourteen appears a ridiculously early one at which to enter the university; but in many cases, as for instance in that of a late president of the Royal Society, Lord Kelvin, it is amply justified in its results. There are many boys who

develop early, and whom it is unfair to measure by the uniform standard of a public school.

Graham's teacher of chemistry was Dr. Thomas Thomson, a man of European reputation. It was in his textbook of chemistry that Dalton's atomic theory was published, before its author had committed his own ideas to the press; and he was a man who maintained the liveliest interest in his science, and whose teaching was most stimulating. His teacher of physics, Professor Meikleham, was also, I have heard, an attractive lecturer; and during his student career, Graham devoted much attention to physics and to mathematics. At the end of his student career, however, Graham had an unfortunate difference of opinion with his father, who had designed him for the Church; with that reserve which is frequently a characteristic of the Scottish nature, neither had made the other aware of his wishes in the choice of a profession; and having made the discovery, with that 'dourness,' also characteristic of the race, neither would yield up his will to the other. Graham therefore left his native city, and pursued his studies in Edinburgh, kept from want by the self-sacrifice of his mother and his sister Margaret, for his father had cut off supplies. There he studied with Dr. Hope, the discoverer of strontium, working diligently the while at mathematics and physics, and so preparing himself for his life-work. Before his student days were over, however, he had begun to earn a little money; and it is recorded that the first six guineas which he earned were spent in presents for his mother and sister.

Having returned to Glasgow, and started a small private laboratory, it was not long before he was asked to become lecturer in the Mechanics' Institute, taking the place of Dr. Clark, the inventor of the process for softening water, who had been appointed to the Chair of Chemistry at

Aberdeen. And in 1830, he succeeded Ure, the author of the *Dictionary of Chemistry*, as professor in 'The Andersonian University,' an institution which had been founded in rivalry to the University of Glasgow, towards the end of the eighteenth century.

In 1837, Edward Turner, the Professor of Chemistry at the then newly founded University of London, now University College, died; and Graham was chosen from among many candidates as his successor. He was much elated at the change, and in a letter to my grandmother (for he was an intimate friend of the family), he tells her that he has suddenly risen to affluence, being in receipt of the fees of no fewer than 400 students who attended his lectures!

Graham was neither a fluent nor an elegant lecturer; but his accuracy, his conscientiousness, the philosophical method in which he treated his subject, and his enthusiasm for his science are said to have proved very attractive to his audience, and without doubt contributed to fill his classroom. The same characteristics are to be noted in his textbook, which I venture to think is the best textbook on chemistry ever written, although it is now completely out of date. No longer republished in English, it still survives in Germany, under the name of 'Graham-Otto.'

Until 1854, Graham retained his Chair at University College; but in that year, Sir John Herschel resigned his office as Master of the Mint, and Graham was chosen to occupy that position, held by so many men of eminence, foremost among whom was Sir Isaac Newton. During his tenure of the office, Graham's conscientiousness proved a sore thorn in the side of the minor officials; and he had a hard struggle to introduce necessary reforms. His strength of character, however, stood him in good stead; and after some years of active combat, he left the field

victorious, with leisure to resume the scientific wor which the state of warfare had interrupted. In th office he remained until his death, which took place i 1869.

Unlike Davy, Graham was of a modest and retirin disposition. His gentleness endeared him to all tho whom he admitted within the circle of his friends; an his calm judgment rendered him an invaluable counsello Yet he received his full meed of honour; he was the fir president of the Chemical Society; a Fellow of the Roy Society; the 'Keith' Medallist of the Royal Society Edinburgh; he twice received a Royal Medal of th Royal Society of London, and in 1862 the Copley Meda given as the reward of a life successfully devoted t scientific discovery; he was a Corresponding Member the Institute of France; and he received from tha august body the *Prix Jecker*.

Graham's scientific work admits of division into tw groups, one relating to the physical behaviour of gase and liquids, and the other to the constitution of salt Besides papers on these subjects, he published a numbe of miscellaneous papers.

In the second of these groups, his earliest communica tion was on the existence of compounds containin alcohol of crystallisation, analogous to the well-know water of crystallisation. The analogy between water an alcohol was thus shown; an analogy which, in the hand of his successor Williamson, played an important part i the development of modern views on the constitution c the carbon compounds, and indirectly on the whole c chemistry. In 1833, Graham published his remarkabl memoir on the phosphoric acids, in which he argued tha as alcohol could replace water in hydrated salts, so wate could replace bases, in such salts as the phosphates The acids of phosphorus had previously been a puzzle t

chemists. Graham proved that orthophosphoric acid consists of a compound of the anhydride, P_2O_5, with three molecules of water, and that each molecule is capable of replacement by the oxide of such a metal as sodium; that pyrophosphoric acid may be regarded as composed of a molecule of anhydrous phosphoric acid with two molecules of water, each of which is replaceable by an oxide; and that metaphosphoric acid is to be represented as a compound of one molecule of anhydride with one molecule of water. The general term, which came to be used for this behaviour of acids was basicity, and an acid was termed monobasic, dibasic, or tribasic, according as it was capable of uniting with one, two, or three molecules of base; yet it might contain the same anhydrous oxide in each case. These views of Graham's made it possible to account for the fact, at that time most mysterious, that on mixing nitrate of silver, with its neutral reaction, with alkaline phosphate of sodium, an acid liquid was the result. These experiments of Graham's paved the way for the later theory, that acids are salts of hydrogen. In Graham's language, the three phosphoric acids were 'terphosphate, biphosphate, and phosphate of water'; for he understood by the term 'phosphoric acid' what we nowadays name phosphoric anhydride. The word phosphate, however, is now applied to the group PO_4, and hence the name phosphates of hydrogen. Graham was the first to recognise that (to quote his own words) 'when one of these compounds (the phosphoric acids) is treated with a strong base, the whole or a part of the water is supplanted, *but the amount of base in combination with the acid remains unaltered.*' We should now say, 'the whole or a part of the hydrogen of the acid is supplanted, but the total number of atoms of hydrogen plus metal in the salt remains unaltered.'

Continuing the train of ideas aroused by his researches

on the phosphoric acids, Graham next advanced the suggestion that certain salts may be substituted, molecule for molecule, for water of crystallisation. Thus, sulphate of zinc ordinarily crystallises with seven molecules of water, forming the heptahydrate, $ZnSO_4.7H_2O$. It is possible to replace one of these molecules of water with a molecule of potassium sulphate, obtaining the double salt, $ZnSO_4.K_2SO_4.6H_2O$. It appeared, too, with this and similar salts, that six molecules of water may be expelled at a lower temperature than the seventh, which may be supposed to be the one which is replaced by the potassium sulphate in the double salt.

Experiments were also made on the heat evolved on neutralising bases with acids, and on the solution of salts in water. Such experiments on salt were carried on until 1843.

But Graham had all the while another set of researches in progress, in which he attempted to arrive at some definite knowledge regarding the constitution of matter. Recognising that the gaseous state represents matter in a simpler condition than that of liquid or solid, his experiments were largely directed towards elucidating the properties of gases. These experiments were started in 1836. From an observation of Doebereiner's, that in a cracked cylinder, containing hydrogen, and standing over water, more gas escaped than entered, so that the level of the water rose in the cylinder, Graham was led to make his experiments on the diffusion of gases, and also on the rate of the escape of gases through narrow openings. Both sets of experiments led to the same law, viz. that the rates of escape are inversely proportional to the square roots of the densities of the gases. Under equal physical conditions, hydrogen moves four times as quickly as oxygen, which is sixteen times as heavy as the former. And since the densities are proportional to the weights of

the molecules, it follows that a molecule of hydrogen moves through space four times as rapidly as a molecule of oxygen. This law was confirmed by measurements made on many other gases. These experimental researches of Graham's have been one of the chief supports of the kinetic theory, devised long afterwards, on the assumptions that the pressure of gases is due to the impacts of their molecules on the walls of the containing vessel, and that their temperature is to be ascribed to the rate of motion of the molecules.

Much later, in 1849, Graham investigated the rate of flow of gases through narrow tubes, and obtained results which have also been found of incalculable service to the theory of gaseous matter.

A few years later, in 1851 and 1852, Graham published investigations on the diffusion of liquids, a subject following close on the lines of his former work on the diffusion of gases. His plan of experiment was as simple as it was well adapted to furnish the information sought. A wide-mouthed bottle was filled with the solution of a salt, and placed inside a wider jar; the jar was then carefully filled with water, care being taken not to disturb the level of the solution in the bottle. The apparatus was then left to itself for a considerable time. It was found that the salt did not stay within the bottle, but gradually escaped into the jar. The amount escaping in different times and at different temperatures was measured.

Experiments made on a great variety of substances soon revealed the fact that some substances escape much more rapidly than others. For instance, Graham found that 69 parts of sulphuric acid, 58 of common salt, 26 of sugar, 13 of gum-arabic, and only 3 of egg-albumen escape in equal times, other circumstances being equal. Some other substances, such as potassium and ammonium chlorides, potassium and ammonium nitrates, magnesium

and zinc sulphates, take equal times to diffuse. Moreover, some salts may be decomposed into their constituents by diffusion; among these are ordinary alum, where the more easily diffusible potassium sulphate passes away from the less quickly diffusing aluminium sulphate. And even potassium sulphate itself shows signs of yielding potassium hydroxide and sulphuric acid on diffusion.

It was known that a solution, placed on the outside of a porous diaphragm, on the inside of which was pure water, tended to pass through the septum; and if the inner vessel, containing the water, were fitted with a pressure-gauge, the pressure would rise in the interior. This pressure had been named 'osmotic pressure.' Graham attempted to connect this phenomenon with diffusion, but found that ordinary salts, as well as sugar, tannin, alcohol, urea, and similar bodies, had little effect in raising pressure. On the other hand, osmotic phenomena were well marked when strong acids, or tartaric, citric, or acetic acids, were present in the cell. In all cases of osmotic pressure, it was found that the porous cell was strongly attacked, and Graham was inclined to ascribe the phenomenon to chemical action. It is in all probability due to the fact that such diaphragms present very little of what we now term 'semi-permeability' to the salts in question.

From the year 1852 to the year 1861, Graham's duties at the Mint absorbed nearly all his time, so that there is a long gap in the series of his publications. But in the latter years he published the results of experiments on the transpiration of liquids, a subject which has lately been successfully treated by numerous investigators. And with his practical bias, Graham devised a plan of applying osmotic phenomena to the separation of crystalline substances, which easily pass through a

porous diaphragm, such as the common acids and salts, from 'colloid' or gum-like substances, the rate of passage of which is much slower. Especially useful was this process for the separation of poisons such as the alkaloids and metallic salts from the contents of the stomach in medico-legal inquiries.

Time allows me only to mention Graham's most interesting experiments on the absorption of gases by metals, and the passage of hydrogen through a thin sheet of palladium; the retention of hydrogen by palladium led him to surmise that the metallic substance was a true alloy of palladium, with metallic hydrogen, and to form the theory that hydrogen itself should be ranked among the metals. He even tried to impress the view by terming the element 'hydrogenium,' in consonance with the nomenclature of most metals.

But I must conclude this imperfect sketch of Graham's work, trusting that what I have said may induce some of my readers to make acquaintance with it at first hand. Graham's conscientiousness in all he did, his enthusiasm, and his great ability render his style in writing a most fascinating one; and his papers will always remain a model to those who publish on similar subjects. He possessed a truly philosophical mind; and in this he more resembled Boyle, than Cavendish or Davy. Indeed, it may be guessed that if Graham had lived in the seventeenth century, and Boyle in the nineteenth, the results of their labours would not have differed very widely from those which bear their respective names.

Contrasting Graham's character with those of Cavendish and Davy, it may be said that while Cavendish carried his devotion to science to such a height that it deprived him of the ordinary pleasures of a human being, and while Davy took perhaps too prominent a part in the world of fashion

to escape the accusation of 'playing to the galler
Graham pursued a happy mean, beloved by the few who
he chose for his intimate friends, and esteemed ar
respected by all. Of him, as of Faraday, it might hav
been said with no shade of misgiving, 'He was a good ar
a true man.'

JOSEPH BLACK: HIS LIFE AND WORK

THERE are some natures so happily constituted that they escape many of the trials which beset most men. Marcus Aurelius thanked his adopted father for having taught him the advantages of 'a smooth and inoffensive temper; constancy to friends, without tiring or fondness; being always satisfied and cheerful; reaching forward into the future, and managing accordingly; not neglecting the least concerns, but all without hurry, or being embarrassed.' Such a character had Joseph Black. Dr. Robison, the editor of his lectures, his successor in Glasgow University, and his biographer, wrote: 'As he advanced in years his countenance continued to preserve that pleasing expression of inward satisfaction, which, by giving ease to the beholder, never fails to please. His manner was perfectly easy and unaffected and graceful. He was of most easy approach, affable, and readily entered into conversation, whether serious or trivial. His mind being abundantly furnished with matter, his conversation was at all times pertinent and agreeable. He was a stranger to none of the elegant accomplishments of life.' His friend Dr. Ferguson said of him: 'As Dr. Black had never anything for ostentation, he was at all times precisely what the occasion required, and no more. Never did any one see Dr. Black hurried at one time to recover matter which had been improperly neglected on a former occasion. Everything being done in its proper season and place, he ever seemed to have leisure in store; and he was ready to receive his friend or acquaintance, and to take his part with cheerfulness in any con-

versation that occurred.' His successor, Dr. Thomas Thomson, found Dr. Robison's estimate of Black's character so just that he appropriated it almost verbatim in his *History of Chemistry* without the formality of quotation marks.

His pupil, Henry Brougham, one of the founders of the college in which I have the honour to hold a chair, portrays him in his *Philosophers of the time of George III.* as 'a person whose opinions on every subject were marked by calmness and sagacity, wholly free from both passion and prejudice, while affectation was only known to him from the comedies he might have read. His temper in all the circumstances of life was unruffled. . . . The soundness of his judgment on all matters, whether of literature or of a more ordinary description, was described by Adam Smith, who said he "had less nonsense in his head than any man living."' Brougham, writing as an old man, said: 'I love to linger over these recollections, and to dwell on the delight which I well remember thrilled me as I heard this illustrious sage detail the steps by which he made his discoveries, illustrating them with anecdotes sometimes recalled to his mind by the passages of the moment, and giving them demonstration by performing before us the many experiments which had revealed to him first the most important secrets of nature. Next to the delight of having actually stood by him when his victory was gained, we found the exquisite gratification of hearing him simply, most gracefully, in the calm spirit of philosophy, with the most perfect modesty, recount his difficulties, and how they were overcome; open to us the steps by which he had successfully advanced from one part to another of his brilliant course; go over the same ground, as it were, in our presence, which he had for the first time trod so many long years before; hold up perhaps the very instruments he had then used, and act over

again the same part before our eyes which had laid the deep and broad foundations of his imperishable renown. Not a little of this extreme interest certainly belonged to the accident that he had so long survived the period of his success—that we knew there sat in our presence the man now in his old age reposing under the laurels won in his early youth. But take it altogether, the effect was such as cannot well be conceived. I have heard the greatest understandings of the age giving forth their efforts in its most eloquent tongues—have heard the commanding periods of Pitt's majestic oratory—the vehemence of Fox's burning declamation—have followed the close compacted chain of Grant's pure reasoning—been carried away by the mingled fancy, epigram, and argumentation of Plunket; but I should without hesitation prefer, for mere intellectual gratification (though aware how much of it is derived from association) to be once more allowed the privilege which I in those days enjoyed of being present while the first philosopher of his age was the historian of his own discoveries, and be an eye-witness of those experiments by which he had formerly made them, once more performed with his own hands.'

Truly, Scotland in the last half of the eighteenth century was the home of many great men. Adam Smith, the first political economist; David Hume, the historian; James Hutton, the geologist; and James Watt, the engineer: all these were intimate friends of Black's, and each in his way was an originator of the first order. And it is my pleasant task to present to you an account of Black's discoveries and their consequences, and to attempt to show that his work began a new epoch for chemistry and physics.

There is little to tell of Black's early history; nor, indeed, was his life even remotely adventurous. His career may be told in a few words.

Joseph Black was born on the banks of the Garonne, near Bordeaux, in 1728. His father, John Black, was a native of Belfast, descended from a Scottish family which had settled there; he resided at Bordeaux, where he carried on a business in wine; he was an intimate friend of President Montesquieu. Joseph was one of thirteen children, of whom eight were sons. In 1740, at the age of twelve, he was sent to school in Belfast; and like many other boys of the north of Ireland, he crossed to Glasgow to attend its University, for in those days, of course, Queen's College, Belfast, had not been founded. This was in the year 1746. Dr. Robison mentions letters from Mr. Black to his son Joseph, from which it would appear that he was in every respect a satisfactory son and a diligent student. He received a general education; we find, at least, that he could write good Latin; and he was taught ethics by Adam Smith. His leanings for natural science, however, were probably encouraged by his intimate friendship with the son of the Professor of Natural Philosophy, Dr. Robert Dick, later successor to his father in the chair, who, unfortunately, occupied it only a few years, for he was early cut off by death. Black also owed much to Cullen, of whom a very interesting account is given by Thomas Thomson in his *History*. Cullen was Lecturer in Chemistry in the University of Glasgow from 1746 to 1756; and in 1751 he was appointed Professor of Medicine; at that time, and, indeed, until Thomas Thomson taught chemistry, that subject was taught only by a lecturer. Thomson attributes to Cullen a singular talent for arrangement, distinctness of enunciation, vivacity of manner, and profound knowledge of his science—in short, enthusiasm—qualities which made him adored by his students. He took especial pains to gain their friendship by frequent social intercourse with them, and no doubt early recognised Black's great promise. Cullen's single contribution to

chemico-physical literature dealt with the boiling of ether on the reduction of pressure, and its growing cold during the process. The reason of this behaviour, however, was later discovered by Black, for Cullen confined himself to recording the observation. It was not long before Black rendered help to Cullen as his assistant; and Black's name was frequently quoted by Cullen in his lectures as an authority for certain facts.

Black's methodical habits led him to keep a sort of commonplace book, in which not merely the results of his experimental work was entered, but also notes on medicine, jurisprudence, or matters of taste; and he practised 'double entry,' for he also kept separate journals in which these notes were distributed according to their subjects. From these notebooks the dates of his most important discoveries can be traced.

Chemistry, in these days, was handmaid to medicine; the influence of the iatro-chemists, founded by Paracelsus, still held its sway, although certain bold investigators—among them Boyle, Mayow, and Hales—a century before, had shaken themselves free from its thraldom. And the lectureship on chemistry in Glasgow was regarded as a step to a more remunerative position, and was held, along with the Crown professorship of medicine, by Cullen from 1751 to 1756. It was probably owing to Cullen's advice that Black went to Edinburgh in 1750 or 1751 to finish his medical studies; perhaps another reason may be found in his having had a cousin in the University, Mr. James Russel, as Professor of Natural Philosophy, with whom he lived. There he took the degree of doctor of medicine in 1754. It is true that he might have graduated in Glasgow three years earlier; but no doubt his thoroughness made him wish to offer a thesis worthy of praise, and it was this thesis which established his reputation. More of this hereafter.

In 1756 Dr. Cullen was called to fill the Chair of Chemistry in Edinburgh, and Black, who had been practising as a physician since he had graduated, was called to succeed him in the Chair of Anatomy and the lectureship in Chemistry; for his reputation in the subject which he had made his own was even then a high one. Black did not retain the Chair of Anatomy for long, however; his tastes lay more in the direction of medicine; and with the concurrence of the University he and the professor of medicine exchanged chairs. While he held these offices he also engaged in medical practice; and Robison says that his countenance at that time of life—he was then about thirty-two—was equally engaging as his manners were attractive; and in the general popularity of his character he was in particular a favourite with the ladies. No one, so far as we know, was singled out by his preference; and to the end of his days he remained unmarried. It appears that the ladies regarded themselves as honoured by his attentions, and we are told that these attentions were not indiscriminately bestowed, but exclusively on those who evinced a superiority in mental accomplishments or propriety of demeanour, and in grace and elegance of manners.

In 1766, Dr. Cullen exchanged the Chair of Chemistry at Edinburgh for that of Medicine; and with one accord University and town united in calling Dr. Black to the vacant chair. Indeed, in 1756, he had been recommended for the chair by the University; but the Town Councillors who were the electors did not agree with the recommendation, and Cullen was appointed. Now, however, unanimity prevailed, and Black removed to Edinburgh, where he spent the rest of his days.

From this date, he devoted himself to tuition, and spared no pains to make his lectures attractive and useful. He illustrated them by numerous experiments.

Robison tells us that, 'while he scorned the quackery of a showman, the simplicity, neatness, and elegance with which they were performed were truly admirable.' And Brougham also praises his manipulation. 'I have seen him,' he writes, 'pour boiling water or boiling acid from one vessel to another, from a vessel that had no spout into a tube, holding it at such a distance as made the stream's diameter small, and so vertical that not a drop was spilt. The long table on which the different processes had been carried on was as clean at the end of the lecture as it had been before the apparatus was planted upon it. Not a drop of liquid, not a grain of dust remained.'

Black had a profound influence on the attitude of the Edinburgh public towards science. The reputation which he established as a lecturer induced many to attend his lectures without any particular wish to learn chemistry, but merely to enjoy an intellectual treat; and it became the fashion to hear him.

The study of the chemistry of gases, after Black's discovery of carbonic acid, made rapid progress; but Black did not take part in its advance. His health had never been good; he was very subject to dyspepsia; and on several occasions his lungs or his bronchiæ appear to have narrowly escaped being affected, for he was troubled with spitting of blood. But he had learned the lesson—γνῶθε σεαῦτον—know thyself; and he regulated his exercise and his diet with the result that he lived a quiet, and a fairly long life. 'Happy is the nation that has no history'; and Dr. Black's uneventful life was passed in happiness. He held his chair for more than thirty years, and grew old gracefully, living amongst many intimate friends. He at one time acquired a reputation for parsimony; but Brougham, while suggesting a reason for this report, namely that he kept a pair of scales on his study table

with which he used to weigh the guineas paid in as fees, defends this perhaps somewhat curious practice, and refutes the imputation; and Robison, who also alludes to it, states in a footnote that he could give more than one or two instances in which a great part of Black's fortune was at risk for a friend.

As his strength decreased, the care of his health occupied more and more of his attention; he became more and more abstemious in his diet. One of his intimate friends, Dr. Ferguson, gives the following account of his death, one worthy of such a calm and placid philosopher: 'On the 26th November 1799, and in the seventy-first year of his age, he expired, without any convulsion, shock, or stupor to announce or retard the approach of death. Being at table, with his usual fare, some bread, a few prunes, and a measured quantity of milk, diluted with water, and having the cup in his hand when the last stroke of his pulse was to be given, he had set it down on his knees, which were joined together, and kept it steady with his hand in the manner of a person perfectly at ease, and in this attitude expired, without spilling a drop, and without a writhe in his countenance, as if an experiment had been required to show his friends the facility with which he departed.'

He left more money than any one thought he could have acquired in the course of his career. His will was a somewhat fantastic one; he divided his property into ten thousand shares; and he distributed it among numerous individuals in shares or in fractions of shares, according to his conception of their needs or deserts.

A tale is told in Kay's *Edinburgh Portraits* of Black and Hutton, who were almost inseparable cronies. Having had a disquisition as to the waste of food, it occurred to them that while testaceous marine animals were much esteemed as an article of diet, those of the land were neglected;

they resolved to put their views in practice, and having collected a number of snails, had them cooked, and sat down to the banquet. Each began to eat very gingerly; neither liked to confess his true feelings to the other. 'Dr. Black at length broke the ice, but in a delicate manner, as if to sound the opinion of his messmate: "Doctor," he said, in his precise and quiet manner, "Doctor, do you not think that they taste a little—a very little queer?"—"Queer,—queer indeed!—tak them awa', tak them awa'!" vociferated Dr. Hutton, starting up from table, and giving vent to his feelings of abhorrence.'

The portraits of the subject of this biography reveal Black as possessing a calm, contemplative nature; but Kay's caricatures indicate that he could take a somewhat humorous view of life, and perhaps might even display a vein of caustic sarcasm. A portrait of him while lecturing may well have been sketched, we may suppose, while he was making scathing comments on the objections raised by a German chemist named Meyer to his doctrine of causticity, which 'that person,' as Brougham tells us, 'explained by supposing an acid, called by him *acidum pingue*, to be the cause of alkaline mildness. The unsparing severity of the lecture in which Black exposed the ignorance and dogmatism of this foolish reasoner cannot well be forgotten by his hearers.' It appears to me, however, that Meyer's theory cannot have been correctly stated by Brougham (for it is remarkably like Black's own explanation), or must have been misunderstood by Black. Another of Kay's portraits exhibits Black and Hutton, under the title of 'The Philosophers'; and here again the caricaturist has made it obvious that Black could appreciate a joke. A third portrait represents him taking a gentle walk; it conveys an idea of his appearance in his fifty-ninth year.

The portrait of Dr. Cullen, Black's predecessor both in Glasgow and Edinburgh, and his life-long friend is also given by Kay. Cullen died in 1790, at the age of eighty-one.

In the olden days it was considered quite as marvellous that a gas could be made to occupy a small volume, or that 'air' could be produced in quantity from a stone, as that an Arabian 'djinn' of enormous size and ferocious mien could issue from a bottle, as related in the 'Tale of a Fisherman,' one of the charming stories of the *Arabian Nights Entertainments.* It is true that in the middle of the seventeenth century Robert Boyle had enunciated his famous discovery, 'Touching the Spring of the Air'; in which he proved that the greater the pressure to which a gas is exposed the smaller the volume it will occupy. But however great the pressure, Boyle's air remained air. It might have been thought that the boiling of water into steam should have convinced men that a liquid, at least, could be changed into a gas; but the fact that steam changed back to water probably prevented attention being paid to its comparative large volume while hot. It was Black's discovery of the production of carbonic acid gas from marble, or as he named it, 'fixed air,' which first directed notice to this possibility of the production of a gas from a solid; and further, the peculiar property of this gas—its power of being fixed—was one which completely differentiated it from ordinary air. Stephen Hales, the botanist, it is true, had distilled many substances of vegetable, animal, and mineral origin; among them he treated many which must have produced impure hydrogen, marsh-gas, carbonic acid gas, and oxygen; but Hales contented himself with measuring the volume of gases obtained from a known weight of material, without concerning himself about their properties. And as the result of many experiments, he concluded that 'our atmosphere

is a *chaos*, consisting not only of elastick, but also of unelastick air-particles, which in plenty float in it, as well as the sulphureous, saline, watry, and earthy particles, which are no ways capable of being thrown off into a permanently elastick state, like those particles which constitute true permanent air.' This was the current belief as regards the nature of air.

The cause which gave rise to Black's famous research is a curious one. Sir Robert Walpole, as well as his brother Horace, afterwards Lord Walpole, were troubled with the stone. They imagined that they had received benefit from a medicine invented by a Mrs. Joanna Stephens; and through their influence she received five thousand pounds for revealing the secret, which was published in the *London Gazette* on the 19th June 1739. It was described as follows:—

'My medicines are a Powder, a Decoction, and Pills. The powder consists of Egg-shells[1] and Snails,[2] both calcined. The decoction is made by boiling some Herbs[3] (together with a Ball, which consists of Soap,[4] Swines'-Cresses, burnt to a Blackness, and Honey) in water. The Pills consist of Snails calcined, Wild Carrot seeds, Burdock seeds, Ashen Keys, Hips and Hawes, all burnt to a Blackness, Soap and Honey.'

Dr. Cullen and his colleagues held opposing views as to the efficacy of such quaint and caustic remedies; and it was with the object of discovering a 'milder alkali,' and bringing it into the service of medicine, that Black began

[1] 'Egg-shells and Snails calcined in a crucible surrounded with coal for 8 hours. Then left in an earthenware pan to slake in a dry room for 2 months. The Shells thus become of a milder taste, and fall into powder.'

[2] 'Snails left in a crucible until they have done smoaking, then rubbed up in a mortar. Take 6 parts of Egg-shell to 1 of Snail-powder. Snails ought only to be prepared in May, June, July, and August.'

[3] 'Herbs of decoction: Green Chamomile, Sweet Fennel, Parsley, and Burdock; leaves or roots.'

[4] 'Soap: Best Alicant Soap.'

his experiments on magnesia. They are described in a paper entitled 'Experiments upon Magnesia Alba, Quicklime, and some other Alcaline Substances'; it was the chemical contents of his thesis for the degree of M.D., which he took at Edinburgh in 1754; he had been making the experiments since 1752. The actual thesis was in Latin: 'De Humore Acido a Cibis orto, et Magnesia Alba'; the pamphlet was published in the following year.

The medicines in vogue as solvents of the urinary calculus were all caustic; the *lapis infernalis*, or caustic potash, and the ley of the soap-boilers, or caustic soda. These substances are made from mild alkali, or carbonates, by boiling their solutions with slaked lime, itself produced by slaking quicklime with water. Now quicklime is formed by heating lime-stone in the fire; it thereby acquires its burning properties, or causticity; and this it was supposed to derive from the fire, of which it absorbed, as it were, the essence. The act of boiling the mild alkalies with lime was supposed to result in a transference of this educt of fire to the alkalies, which themselves became caustic. Lime-water, or a solution of caustic lime was used as a solvent for the calculus; and it was an attempt to produce a less caustic solvent from Epsom salts that induced Black to begin his researches.

As his notes show, Black began by holding the old view. He attempted to catch the igneous matter as it escaped from lime, as it becomes 'mild' on exposure to the air: he appears to have made some experiment with this view; but his comment was, 'Nothing escapes—the cup rises considerably by absorbing air.' Two pages further on in his notebook he records an experiment to compare the loss of weight sustained by an ounce of chalk when it is calcined with its loss when dissolved in 'spirit of salt,' or hydrochloric acid; and he then evidently began to suspect the reason of 'mildness' and 'causticity.'

Another memorandum, a few pages later, shows that he had solved the mystery. 'When I precipitate lime by a common alkali there is no effervescence. The air quits the alkali for the lime, but it is not lime any longer, but c.c.c. It now effervesces, which good lime will not.'

But we must trace the chain of reasoning which led him to come to this conclusion.

Having prepared 'mild' magnesia by mixing Epsom salt or sulphate of magnesia with carbonate of potash, or 'pearl-ashes,' he found that it is 'quickly dissolved with violent effervescence or explosion of air by the acids of vitriol, nitre, and of common salt, and by distilled vinegar'; that the properties of these salts—the sulphate, nitrate, chloride, and acetate of magnesium—differ greatly from those of the common alkaline earths; that when boiled with 'salt-ammoniac,' or chloride of ammonium, volatile crystals of smelling-salts were deposited on the neck of the retort, which, on mixing with the chloride of magnesium remaining in the retort, reproduced the 'mild' magnesia; that a similar effect is produced by boiling 'mild' magnesia with 'any calcareous substance'; while the acid quits the calcareous salt to unite with the magnesia, 'mild' magnesia is again precipitated on addition of a dissolved alkali.

On igniting 'mild' magnesia, it changed into a white powder, which dissolved in acids without effervescence. And the process of ignition had deprived it of seven-twelfths of its weight. Black next turned his attention to the volatile part; he attempted to restore it by dissolving the magnesia in a sufficient quantity of 'spirit of vitriol' or dilute sulphuric acid, and separated it again by the addition of alkali. The resulting white powder now effervesced violently with acids, and 'recovered all those properties which it had lost by calcination. It had acquired besides an addition of weight nearly equal to

what had been lost in the fire; and as it is found to effervesce with acids, part of the addition must certainly be air.'

Black here made an enormous stride; he had weighed a gas in combination. He argues further: 'It seems therefore evident that the air was forced from the alkali by the acid, and lodged itself in the magnesia.' We may represent the change diagrammatically thus:

Magnesia ↘ ↗ Alkali→Vitriolated alkali.
Spirit of vitriol ↗ ↘ Air→Mild magnesia.

The next step was to try whether mild magnesia lost the same weight on being mixed with acid as it did when heated in the fire. But owing probably to the solubility of the fixed air in the water, a much less loss was found on dissolving the magnesia (35 grains out of 120) than by heating it (78 grains out of 120). The amount of acid required to expel the fixed air was, however, practically the same as that required to dissolve the *magnesia usta*, or heated magnesia (267 and 262 grains).

Turning his attention next to chalk, he dissolved some in muriatic acid, and having precipitated with fixed alkali no difference could be detected between the recovered and the original chalk. He had thus first separated the fixed air from the chalk, and then recombined the two. These experiments led Black to conclude that fixed air must be of the nature of an acid, for it converts quick-lime—the acrid earth, as he termed it—into crude lime, or mild earth, the mildness being due to its union with fixed air.

The explanation is thus given of the curious fact that mild magnesia, mixed with lime-water, gives pure water; for the fixed air leaves the magnesia and unites itself to the lime, and both the *magnesia usta* and the chalk which are formed are insoluble in water. And the action

of quick-lime in causticising alkali is similarly explained by its removing the fixed air from the alkali, thus rendering the latter caustic, while itself becoming mild.

Reasoning further, Black foresaw that caustic alkali, added to Epsom salt or vitriolated magnesia, should give a precipitate of magnesia which should not effervesce with acids, for here fixed air is excluded; and, also, that caustic alkali should separate from acids lime in the quick state, only united with water.

Similar experiments of treating chalk with acids and heating it, which had been performed with magnesia, showed similar results.

But it had yet to be demonstrated that fixed air did not share the properties of ordinary atmospheric air. So Black placed four fluid ounces of lime-water, as well as four ounces of common water, under the receiver of an air-pump, and exhausted the air; air rose from each in about the same quantity; it therefore appeared that the air which quick-lime attracts is of a different kind from that which is mixed with water. Quick-lime does not attract air when in its most ordinary form, but is capable of being joined to one particular species only, 'which is dispersed through the atmosphere, either in the state of a very subtle powder, or, more probably, in that of an elastic fluid. To this I have given the name of *fixed air*, and perhaps very improperly; but I thought it better to use a word already familiar in philosophy than to invent a new name, before we be more fully acquainted with the nature and properties of this substance.'

The next step was to examine the nature of caustic alkali, and to prove whether it gained weight on being made 'mild.' This was achieved indirectly, by finding the amount of acid required to neutralise the same weight of caustic alkali, and 'salt of tartar'—what we know as potassium carbonate. Six measures of acid were required

to saturate the former, and five the latter; and Black was very near the truth; indeed his error was only about four per cent. He proved, by addition of sulphuric acid, that the caustic alkali contained no lime, and therefore that its causticity was not due to an admixture of that substance.

To prove that lime-stone, or magnesia, 'loses its air' when dissolved in an acid, but regains it on addition of a mild alkali, the acid in which the lime was dissolved passing to the alkali, Black added caustic ley to a solution of Epsom salt, the result being a precipitate of magnesia; this dissolved in vitriol without effervescence, showing that no fixed air had taken part in the change. He also, on adding caustic alkali to a solution of chalk in spirit of salt (or hydrochloric acid), produced lime, which on being dissolved in water produced lime-water, indistinguishable from that produced from quick-lime and water. He goes on to say that 'had we a method of separating the fixed alkali from an acid, without at the same time saturating it with "air" we should then obtain it in a caustic form.' It can be done, it is true, by heating nitre with charcoal, but the alkali is then found saturated with air; and again, by heating the alkali-salts of vegetable acids, the same occurs. Black conjectures that the fixed air must be derived either from the nitre or the charcoal in the first case (indeed it is derived from both, the nitre supplying the oxygen to the carbon); and in the second, he remarks that the vegetable acid is not really separated, but rather destroyed by the fire. How nearly he came to the discovery that fixed air is produced from carbon!

Such was Black's research on fixed air. And now having shown that a gas can be retained by a solid, and can be made to escape by treatment with acid or by heat, he attacked somewhat later the problem of the cause of this fixation. He discovered it to be due to what he

termed 'latent' or hidden heat. But his research was not made with this object; the connection of the two was fortuitous, although of a fundamental nature.

Between the years 1759 and 1763, he formed opinions regarding the quantity of heat necessary to raise equally the temperatures of different substances. Boerhaave imagined that all equal portions of space contain equal amounts of heat, irrespective of the nature of the matter with which they are filled; and his reason for this statement was that the thermometer stands at the same height if placed in contact with objects near each other. Here we have a confusion between heat and temperature; and this was perceived by Black, for he pointed out that a distinction must be drawn between *quantity* and *intensity* of heat: the latter being what we now call *temperature*. He quotes Fahrenheit to show that while equal measures of water at different temperatures acquire a mean temperature when mixed, it requires three measures of quicksilver at a high temperature to convert two measures of water at a low temperature to the mean of the two temperatures; and this corresponds to twenty times the weight of the water. Black expressed this by the statement that the capacity for heat of quicksilver is much less than that of water.

But before this, in 1757, Black had made experiments leading up to these views. He had noticed that when ice or any solid substance is changing into a fluid, it receives a much greater amount of heat than what is perceptible in it immediately afterwards by the thermometer. A great quantity of heat enters into it without making it perceptibly warmer. Conversely, in freezing water or any liquid, a large amount of heat comes out of it, which again is not revealed by a thermometer.

He then proceeded to estimate the quantity of heat which had to be absorbed by a known weight of ice in

order to melt it. He hung up two globes side by side, about 18 inches apart, in a large empty hall, in which the temperature remained practically constant; each globe contained 5 ounces; one of ice at 32° F., the other water at 33°. The latter had a delicate thermometer suspended in it. The temperature of the hall was 47° F. In half an hour, the water had attained the temperature 40° F.; and the ice took ten hours and a half to attain the same temperature, that is, twenty-one times as long as the water. The heat, which the ice absorbed during melting was $(40-33) \times 21$ or 147 units; that is, had it been absorbed by the five ounces of water it would have made it warmer by 147°. The temperature of the ice, however, was 8° warmer than its melting-point, after the 21 half-hours; hence 139 or 140 'degrees had been absorbed by the melting ice, and were concealed in the water into which it had changed.'

The method of experiment was next varied. Black weighed a lump of ice, and added it to a weighed quantity of warm water of which the temperature was known. The warm water was cooled to a much lower degree by the melting of the ice, than if it had been mixed with a quantity of water of 32° F., equal in weight to the ice. The quantity of heat absorbed by the ice in melting appeared from this second experiment to have been capable of heating an equal quantity of water through 143° F.

A third experiment was made, in which it was proved that a lump of ice, placed in an equal weight of water at 176°, lowered the temperature of the water to 32°. Now $176-32=144°$—again a similar result. The latent heat of water is therefore about 142 or 143, in Fahrenheit units. The result of the most careful measurements give 79·5° centigrade units, which corresponds with 143° units of Fahrenheit's scale. Curiously enough, this fundamental datum has not yet been determined with the accuracy

which is customary nowadays, and it is still uncertain to one seven-hundredth of its value. Black's determination was a remarkably good one, especially if we consider the crude appliances which he used.

The substance of this research was communicated to the 'Philosophical Club,' or Society of Professors and others in the University of Glasgow in the year 1762, and was expounded yearly by Black in his lectures to his students.

Black suggested to Irvin, his pupil, and afterwards his successor in the Glasgow chair, to determine the latent heat of fusion of spermaceti and bees'-wax; and he found that these substances, too, absorb heat, insensible to the thermometer, on assuming the liquid state. In this manner, he made his thesis general. But in attempting to extend it beyond the case of liquids and solids, he went astray. For example, he imagined that the great rise of temperature, which may even reach redness, caused by the hammering of iron by a skilled smith, was due to the 'extrication of the latent heat of the iron by hammering.' He did not realise that heat can be *produced* from mechanical work; that work can be quantitatively transformed into heat; a discovery made more than eighty years later, by Joule, although it had been anticipated by Count Rumford, and by Sir Humphry Davy, in the beginning of last century.

Similar experiments were made by Black on the latent heat of steam, in which he compared the time required for a known weight of water to rise through a definite interval of temperature when exposed to a constant supply of heat with that required to dissipate the water into steam. But his estimate of 830 units required to evaporate one part of water was not so accurate; the actual figure is 967 units on the Fahrenheit scale. Black cited experiments by Boyle, by Robison, his successor in the Glasgow chair, and by

Cullen, his predecessor, in which the boiling-point of liquids had been found to be lowered by reduction of pressure; he rightly ascribes this to the freer escape of the vapour, and to the absorption of heat by the vapour, and the consequent cooling of the liquid from which it is escaping.

These conceptions of Black's were utilised by his friend James Watt in his work on condensers, and, as every one knows, effected a revolution in the structure of steam-engines, and as a consequence in the whole of our industrial and social life; and further, they were developed by many men of science, until in the hands of the masters—Joule, Clerk-Maxwell, Rankine, James Thomson, and Kelvin, on the physical side, and of Willard Gibbs, the American, on the chemical side—they form the very groundwork of the sister sciences, physics and chemistry.

Black's great chemical discovery that a gas exists which is clearly not a modification of atmospheric air, seeing it can be 'fixed' by alkalies and alkaline earths, led the way to 'pneumatic chemistry,' as it was called, and was followed by the discovery of oxygen by Priestley, of nitrogen by Rutherford, of hydrogen by Cavendish and Watt, and of the more recent discoveries of argon and its congeners, all of them constituents of the atmosphere. In fact, the gases of the atmosphere have been discovered entirely by Scotsmen and Englishmen.[1]

And Black's proof, that the change of a complex compound to simpler compounds, and the building up of a complex compound from simpler ones, can be followed successfully by the use of the balance, has had for its consequence the whole development of chemistry. It is only in the most recent years, since Becquerel observed the effect of uranium ores and salts in discharging an

[1] In justice to the Swede Scheele, it should be said that his discovery of oxygen was contemporaneous with Priestley's.

electroscope, and since Madame Curie discerned one of the causes of the discharge by uranium ore, namely, the existence in it of a new element, radium, and since Rutherford and Soddy's isolation of the gases evolved from radium and from thorium, that a new and more sensitive instrument has been placed at the disposal of chemists in the electroscope. We are at the beginning of a new era. Every discovery of a new principle of research heralds a new departure; and the compound nature of many of the so-called elements begins to appear from their electrical behaviour, in much the same manner as Black demonstrated the decomposability of compounds in the year 1752.

LORD KELVIN

On June 16, 1896, there took place in the University of Glasgow an almost unique ceremony. On that day the jubilee of Lord Kelvin was celebrated; he had been Professor of Natural Philosophy at Glasgow University for fifty years. The Prince of Wales, now King Edward, sent him a letter of congratulation; twenty-eight universities, twelve colleges, and fifty-one learned societies sent delegates with addresses, wishing Lord Kelvin many more years of health and happiness, and mentioning in terms of profound admiration his magnificent achievements in the domain of physics. What were these, and why did they deserve and obtain such universal admiration? To answer that question fully would require a much longer space than is at my disposal; but I shall try to give a short sketch of William Thomson's life and work.

In 1812, James Thomson, William's father, was a teacher in the Royal Academic Institute of Belfast. He was one of the descendants of a number of Scotsmen who emigrated to North Ireland in the seventeenth and eighteenth centuries. He had two sons, James and William, both of whom were born in Ireland, and both of whom became illustrious. When William was eight years old, his father was appointed to the Chair of Mathematics in the University of Glasgow. My father was one of his students; and I remember well his allusions to Professor Thomson's kindliness and sense of humour.

It was his habit to cross-examine his students, at th beginning of each lecture, on the subject of the precedin day's work; and it was customary in his junior class t begin with very elementary questions. One day he aske a certain Highlander: 'Mr. M'Tavish, what do you under stand by a "point"?' The answer was, 'It 's just a dab! Again, Mr. M'Tavish was asked, in the course of the con struction of some diagram: 'What should I do, Mr M'Tavish?' 'Tak a chalk in your hand.' 'And next? 'Draw a line.' Professor Thomson complied, and pausing said: 'How far shall I produce the line, Mr. M'Tavish? '*Ad infinitum!*' was the astounding reply.

At the mature age of ten William entered the universit His training had been wholly in his father's hands; Pro fessor Thomson was clear-sighted enough to recognise tha he had two very remarkable sons. They were brought u on Classics and Mathematics, Logic and Philosophy.

In May 1907, at the annual dinner of the Londo 'Glasgow University Club,' I had the good fortune to hea Lord Kelvin express his views on education. His them was the 'University of Glasgow'; and he commended th universality of the training which it used to give. By th age of twelve, said he, a boy should have learned to writ his own language with accuracy and some elegance; h should have a reading knowledge of French, should b able to translate Latin and easy Greek authors, and shoul have some acquaintance with German. 'Having learne thus the meaning of words,' continued Lord Kelvin, 'a bo should study Logic.' In his charming discursive style, h went on to descant on the advantages of a knowledge o Greek. 'I never found,' he said, 'that the small amoun of Greek I learned was a hindrance to my acquiring som knowledge of Natural Philosophy.' It certainly was no in his case. And it may here be remarked that it is surel a mistake to lay down a hard and fast rule that no yout

ıould enter a college until he has reached the age of 'teen or sixteen; William Thomson took the highest ·izes in Mathematics and Physics before he reached that ;e. It may be said that his precocity was phenomenal; › doubt it was; but it is precisely those boys who are ıique and unlike their fellows who are of value to the ce, and every chance should be given to exceptional lent.

Although William Thomson spent six years at Glasgow niversity, he did not graduate: in those days the aim of student's ambition was not a degree, but the acquisition knowledge. Before he had reached the age of seventeen, › went to Cambridge, where he passed four years. There e examination system was in full swing; and to the sgrace of the examiners, Thomson was not the 'Senior rangler'; he was not regarded as the best mathematician his year; and this, in spite of the remark made by one his examiners, that 'the Senior Wrangler was not fit cut pencils for Thomson.' It is known that success in is examination depends largely on rapidity in writing d on accuracy of memory, rather than on originality; d the tale is told that on Thomson's 'coach,' or tutor, ;ing him why he had spent so much time in answering a rticular question, he replied that he had to think it all t from first principles. 'But it is a problem of your own ;covery,' said the coach. Thomson had to confess that had quite forgotten his own handiwork, and that while his mpetitor had learned the answer by heart, Thomson had d to rediscover the solution. However, he was successful gaining the 'Smith's Prize,' a reward for inventiveness her than memory. That same year, he was elected llow of his College, and had an income of about £200, ich enabled him to continue his studies in France.

While at Cambridge, Thomson was not only a student; always took a keen interest in music, and was president

of the Musical Society; he also carried off the 'Colquhoun sculls' for his excellence as an oarsman. In those days the science of Cambridge was fettered by the bonds which Newton had imposed. It is unfortunate, though perhaps natural, that to the advent of a great man a period of stagnation succeeds. It was thus with the Schoolmen, who subsisted for many centuries on the philosophy of Aristotle; and the science of Cambridge, in 1845, was based on the work of Newton, nearly a century and a half old. Indeed, the spirit was that of Timæus, in Plato's dialogue, who said: 'If we wish to acquire any real acquaintance with astronomy, we shall let the heavenly bodies alone.' In fact, Bacon's advice to proceed by way of experiment and induction had been forgotten. Needless to say, this reproach has long been removed, by the labours of Clerk-Maxwell, Rayleigh, Stokes, and J. J. Thomson. In the 'forties Paris was the home of Fourier, Fresnel, Ampère, Arago, Biot, and Regnault, all physicists and mathematicians of the highest rank; and Thomson spent a year working in Regnault's laboratory, where experiments on water and steam, their densities, pressure, and specific heats, were being carried on with the utmost refinement. During the next year, 1846, the Chair of Natural Philosophy in Glasgow fell vacant, and, to their credit, the Senate of the day advised Queen Victoria to appoint William Thomson, then a youth of twenty-two, as professor. Never was a choice better justified in its results. For Thomson, by example and by precept, trained many students to be a credit to their old university, and carried out in cellars, which served as laboratories, and which were situated almost next door to that in which James Watt invented the condensing engine, almost all his numerous and important investigations.

Thomson was not what would be called a good lecturer; he was too discursive. I doubt whether any man with a

brain so much above the ordinary, so much more rapid in action than the average, can be a first-rate teacher. Certainly, in my own case, I gained much more in my second than in my first year's attendance. But Thomson never allowed the interest of his students to flag; his aptness in illustration and his vigour of language prevented that. Lecturing one day on 'Couples,' he explained how forces must be applied to constitute a couple, and illustrated the direction of the forces by turning round the gas-bracket. This led to a discussion on the miserable quality of Glasgow coal-gas, and how it might be improved. Following again the main idea, he caught hold of the door, and swung it to and fro; but, again, his mind diverged to the difference in the structure of English and Scottish doors. We never forgot what a couple was; but —the idea might have been conveyed more succinctly. He held strong views on the 'absurd, ridiculous, time-wasting, soul-destroying system of British weights and measures'; and in spite of all the efforts of the 'Decimal Association,' we, the Americans, and the Russians remain examples of irrational conservatism in respect of the awkwardness of our systems.

The Cartesian method of locating a point was indelibly impressed on my memory by the following incident: A student, whose position was roughly about the centre of the lecture-room, made that noise so disturbing to a lecturer, yet so difficult to locate, caused by gently rubbing the sole of his foot on the floor. 'Mr. Macfarlane!' said Sir William. Mr. Macfarlane, the *fides Achates*, came, received a whispered communication, and went out of the room. In about ten minutes he returned with a tape-line, and proceeded to measure a length along one wall, on which he made a pencil-mark. He then measured out at right angles another length, and made a chalk-mark on the floor, erecting on it a pointer. 'Mr. Smith, it was you

who made that noise: be so good as to leave the room,' said Sir William. Mr. Smith blushed and retired. Then came the explanation. Mr. Macfarlane had gone below the sloping tier of seats; had accurately diagnosed the precise position of Mr. Smith's erring foot, and had accurately measured the distance from the two walls. These measurements were reproduced in full view of the students, and the advantages of the system of Cartesian co-ordinates were experimentally demonstrated, while justice was satisfied.

Owing to an accident, Sir William was lame; but it did not interfere with his activity of body. Indeed, it lent emphasis to his amusing class demonstration of 'uniform velocity,' when he marched backwards and forwards behind his lecture-bench, with as even a movement as his lameness would permit; and the class generally burst into enthusiastic applause when he altered his pace, and introduced us to the meaning of the word 'acceleration.'

In his laboratory Sir William was a most stimulating teacher, though his methods were not those which have since been introduced into physical laboratories. I remember that my first exercise, which occupied over a week, was to take the kinks out of a bundle of copper wire. Having achieved this with some success, I was placed opposite a quadrant electrometer and made to study its construction and use. I was made to determine the potential difference between all kinds of materials, charged and uncharged; and among others between the external and internal coatings of a child's balloon, black-leaded externally and internally, and filled with hydrogen. Nor was the Professor always prescient. On one occasion I turned the handle of a large electrical machine, while he held a two-gallon Leiden jar by its knob, and charged the outside coating. It was not until it was fully charged that it occurred to one of us that while the jar was quite

safe as long as it was in his hands, it was impossible for him to deposit it on the table without running the risk of an inconveniently heavy shock. Finally, after rapid deliberation, two of us held a towel by its corners, and Sir William dropped the jar safely into the middle; it was then possible to touch the outside without mishap. In short we had little systematic teaching, but were at once launched into knowledge that there is an unknown region where much is to be discovered; and we were made to feel that we too might help to fathom its depths. Although this method is not without its disadvantages—for systematic instruction is of much value—there is much to be said for it. On the one hand, too long a course of experimenting on old and well-known lines, as is now the practice among teachers of science, is likely to imbue the young student with the idea that all physics consists in learning the use of apparatus, and in repeating measurements which have already been made. On the other hand, too early attempts to investigate the unknown are likely to prove fruitless for want of manipulative skill, and for want of knowledge of what has already been done. The best of all possible training, however, is to serve as hands for a fertile brain—the brain of one who knows what he wishes to discover, who is familiar with all that has already been attempted, and who gradually trains his assistant to take part in the thinking as well as in the manipulation. If at the same time the student is made to read, not merely concerning the problem on which he is immediately engaged, but on all branches of his subject, nothing can be better than such stimulating intercourse with an inventive teacher for those who have ability to profit by it.

It is extremely difficult to explain Lord Kelvin's contributions to knowledge to those who have not themselves some acquaintance with its problems. Let me begin by a

quotation from Helmholtz, late Professor of Physics in Berlin, an old and intimate friend of Lord Kelvin: 'His peculiar merit consists in his method of treating problems of mathematical physics. He has striven with great consistency to purify the mathematical theory from hypothetical assumptions, which were not a pure expression of the facts. In this way he has done very much to destroy the old unnatural separation between experimental and mathematical physics, and to reduce the latter to a precise and pure expression of the laws of the phenomena. He is an eminent mathematician, but the gift to translate real facts into mathematical equations, and *vice versâ*, is by far more rare than that to find a solution of a given mathematical problem, and in this direction Sir William Thomson is most eminent and original.' When Lord Kelvin began his work, the equivalence of heat and energy was unrecognised; forces were distinguished as 'conservative' and 'unconservative'; the world was supposed to be filled with subtle fluids and effluvia; and it must have seemed almost hopeless to seek any general explanation of material phenomena. Light, heat, electricity, magnetism, and chemical action were all regarded as distinct 'forces,' each a cause of change. Thomson, and his collaborator Tait, the late Professor of Physics in Edinburgh, in their *Treatise on Natural Philosophy*, did much to emphasise the view that Physics deals with things, not theories; with relations, not with their mathematical expression, equations; and they tried successfully to free the science from the bonds of formal mathematics. They demonstrated that the principle of 'Least Action' is universal; that by its help it is possible to explain the motions of the planets and their satellites, of wheels, lathes, machines of all kinds, of every system of which we can define the moving parts and the forces which act on them.

In 1893 Lord Kelvin gave a discourse on 'Isoperimetric Problems' at the Royal Institution, in which he attempted to describe the nature of this general problem; it is that technically called 'Determining a minimum'; and he began with the task which faced Dido of old—to surround the most valuable piece of land with a cowhide, *i.e.* to draw the shortest possible line around it. A similar problem is, to build a railway-line through undulating country at the smallest possible cost; and one very different in appearance, but related to those already cited, owing to Lord Kelvin's consummate power of discovering analogies between phenomena apparently unconnected, is the condition of stability of water rotating in an ellipsoidal vessel, and a number of similar problems. Kelvin's work on Elasticity is no less far-reaching; in Karl Pearson's great treatise on that subject, no less than one hundred pages are filled with Kelvin's contributions.

Lord Kelvin was also the author of a theory of the nature of the ultimate particles of matter—the atoms. He imagined them to consist of 'vortex rings in the ether,' the ether being conceived as a frictionless fluid, all-present, even filling the interstices between the atoms, or ultimate particles of matter. Vortex rings in air, sometimes made by smokers, are elastic; they cannot be cut without being destroyed; and, in a frictionless fluid, their rotatory motion would be eternal, if once impressed. Recent discoveries may lead to the modification of this theory of the nature of matter; but it has much in its favour.

Kelvin was a strong partisan of Joule's work on the equivalence of heat and work. It was believed up to 1850 that the heat developed on compressing a gas was 'caloric,' squeezed out of the gas, as one might squeeze water out of a sponge; but Kelvin taught that heat must

be due to the motions of the molecules of a gas; and that when the gas is compressed, the impacts of its molecules on the walls of the containing vessel are more numerous, and that the work done in compressing a gas appears as heat, owing to the more numerous impacts of its molecules. Following on this, it was necessary to devise an absolute scale of temperature, and that we also owe to Lord Kelvin. It is based on what is known as the 'Second Law of Thermodynamics'—that heat cannot be transferred from a cold to a hot body without expending work. Following these ideas, Lord Kelvin was led to consider the probable age of the earth, based on an estimate of its original temperature, and the rate at which heat would be lost by radiation. His opinion is that the earth may have been habitable twenty million years ago, but could not have been habitable as long ago as four hundred million years.

The province of electro-magnetism owes very much to Lord Kelvin. It was he who developed the medium suggested by Faraday into a means of representing electromagnetic forces by analogy with the distortion of an elastic solid. After he had worked out in this manner the connection between energy and electro-magnetism, he devised our present system of electrical units—volts, ampères, farads, coulombs, etc., and invented machines to determine their numerical values. If it be permitted to assign their relative importance to his contributions to practical science, this must be pronounced the greatest. Without it the science of electricity would be helpless as commerce without a monetary system, and without weights and measures. His work is the foundation of wireless telegraphy, and of many applications of the electric current. It was he who taught the world how to transmit rapid and trustworthy signals through cables; and he was a pioneer of cable telegraphy. In the old days of cables

attempts were made to ensure rapid signalling by heavy currents; but Kelvin showed that feeble currents, combined with delicate instruments, made the difficulty disappear. His 'siphon recorder' is still used, and cannot well be improved on. A great social and commercial revolution dates from August 1858, when the message was signalled under the ocean, 'Europe and America are united by telegraphic communication. "Glory to God in the highest, and on earth peace, goodwill toward men."' This revolution owed much to Sir William Thomson, who never lost heart and never faltered in the belief that all difficulties would be overcome. His presence on board ship during the laying of the first Atlantic cable directed his attention towards nautical matters; and to him we owe a deep-sea sounding apparatus, and a compass easily corrected for the magnetic deviations produced by the iron or steel used in the construction of ships.

We must not estimate Lord Kelvin's greatness, however, merely by his own discoveries and inventions, great as these are; he has served as a model for many disciples. His sincere and single-minded devotion to truth; his interest in the work of others, and his sympathy with their efforts; his fairness of mind and absence of prejudice; and his straightforward and loving character have raised the ideals of the whole scientific world, and have deeply influenced the best minds in all countries. His idea of 'a treasure of which no words can adequately describe the value' is: 'Goodwill, kindness, friendship, sympathy, encouragement for more work.' It is to such a man that the world owes an eternal debt of gratitude, and he it was for whom no honour that men have it in their power to bestow could be too great. It is pleasant to be able to state that Lord Kelvin's mental energy was unimpaired by his burden of more than eighty years. He was present at the meeting of the British Association at

Leicester in August 1907, and took part in the discussions on the 'Nature of the Atom.' The minds of most men, like their bodies, grow stiff with age and unreceptive of new impressions; but Lord Kelvin's until his latest days had all the vigour and elasticity of a young man's. We may well rejoice that he was spared so long to enrich the world with his wisdom and his inimitable example.

PIERRE EUGÈNE MARCELLIN BERTHELOT
1827-1907 [1]

MARCELLIN BERTHELOT was a native of Paris, born on October 25, 1827, in a flat looking on to the Rue du Mouton, situated in the Place de Grève, now, owing to the activity of Baron Haussmann, the Place de l'Hôtel-de-Ville. His father, a doctor of medicine, was a member of the sect of the Jansenists, a small branch of the Gallic Catholic Church. He was a serious man, impatient with the folly of his *concitoyens*, and somewhat depressed by the poverty and sufferings of his patients. The 'Church of Faith' had its own Liturgy, and the congregation joined in singing psalms and hymns. Many of the *prêtres* were among Dr. Berthelot's patients, and young Berthelot must often have listened to discussions on the attempts, ultimately successful, to substitute the Roman for the Gallic liturgy. Dr. Berthelot was married in 1826, shortly after starting practice. His wife was a lively, bright woman, who transmitted her features to her son.

At that time, Charles the Tenth was on the throne. The allied powers had involved France in a *Gouvernement de Curés*; and it was part of the State Ceremonial to form a procession, which was headed by the Holy Sacrament and the Papal Nuncio, a cardinal in red, from the Tuileries, to Notre Dame and back, and in which the King, the Queen, the Dauphin (who, according to Madame

[1] A notice which appeared in the *Proceedings of the Royal Society* for 1907.

Berthelot *mère,* was able to look behind him without turning his head), and the Court took part. The spectators, under the penalty of sacrilege, were obliged to kneel as the Corpus Christi procession passed. Those who refused were prosecuted and severely punished.

Such a travesty of religion was not to Dr. Berthelot's taste; the *bourgeoisie* was liberal and imbued with the sentiments of Voltaire; and the Berthelot family was of the bourgeois class. During the revolutions of 1830 and 1848, their house commanded a full view of one of the chief scenes of operation, and young Berthelot must have often been a spectator of many a scene of disturbance and violence. Highly developed intellectually, and mentally impressionable, his later convictions were doubtless largely owing to his early surroundings.

That Marcellin resembled his mother in features has already been mentioned. But the resemblance was not merely external; there existed between them the most intimate sympathy. Their favourite promenade was in the Bishop's garden behind Notre Dame, along the Quays with their stalls of flowers, and in the Jardin des Plantes. Their minds were both quick and versatile; they were eagerly interested in all that passed around them, and, as Madame Berthelot used to say (borrowing the simile from one of the invasions which she witnessed), they could both 'drive a Russian team with a sure hand and at a full gallop.' The writer, who knew Berthelot only during his later years—since 1878—never conversed with any one who possessed such rapidity of thought. Given an idea, with his quick discursive mind he would follow out all possible paths and by-ways, seeing the consequences of this assumption and of that, interposing occasionally a quaint remark, not exactly humorous, but *de plaisanterie.* He was a delightful conversationalist, interested and intensely interesting, willing to discuss all possible subjects,

and willing, too, to hear all varieties of view, even those contrary to his own opinion.

His persistence, energy of character, and devotion to duty were inherited from his father. Berthelot used to regret that he had not inherited his mother's optimism. He used to say that when a misfortune overtook her, she had what the French call a *crise de larmes,* soon over and followed by her usual optimistic cheerfulness; that a rainbow generally rose through her tears, and that she became gaily resigned to the incurable evil.

After the demolition of the Rue du Mouton, the family moved to Neuilly, then quite in the country. Renan often looked in on Sundays as a guest at their midday meal. In one of his private letters he tells how Berthelot and he became friends. He had just renounced his clerical orders, and was *maître-répétiteur* in a school, where he led a lonely and melancholy existence, depressed by the mental struggles which he had come through, and far from his family and his native Brittany. One day, a pupil about four years younger than himself accosted him; the talk became intimate, and a friendship with Berthelot was soon formed, destined to endure for life. Their intercourse was frequent; begun early, when both were slender youths, never a year, hardly a month, passed without their seeing each other. Renan used sometimes to poke fun at Berthelot; the tale is told that, passing a cemetery, Renan said to him: 'Là, voici la seule place que tu n'as jamais convoitée.' Such sallies were always received with amusement and good temper. On another occasion, provoked by the remark that his coat was worn with the air of a cassock, Renan retorted: 'What is there in you, Marcellin, that gives you the air of just having left off fighting behind a barricade?' While Berthelot retained his slender form, Renan became very corpulent; Berthelot, nervous and active, maintained to the last his almost

feverish love of work; Renan was meditative—almost a dreamer. It was Berthelot's sad duty to speak of his lost friend when the monument at Tréguier was raised to his memory. He emphasised Renan's lucidity even to the end, his power of work, his great mental activity; the words were applicable with equal force to himself.

Never was there a more devoted couple than Monsieur and Madame Berthelot. After he had ended his brilliant career at the Lycée Henri IV., Berthelot gained the prize of honour at the open competition in 1846. Without any coaching, he passed successively all his degrees—Bachelier, Licencié, and Docteur-ès-Sciences; for the doctorate he presented a somewhat sensational thesis, entitled, 'The Compounds of Glycerine with Acids, and the Artificial Production of the Natural Fats.' While working at this research, he was lecture-assistant (*préparateur*) to Balard, at the Collège de France. In 1861, largely through the influence of Duruy, then Minister of Public Instruction, Berthelot was promoted to the Chair of Organic Chemistry in that institution; and there he remained all his life. In that year he was awarded by the Academy of Sciences the Jecker Prize for his remarkable researches on the artificial production of organic compounds by synthesis, and at the same time the Academy recommended the creation of the special chair which Berthelot filled so long and so illustriously. In his own words: 'Adonné, dès mes débuts dans la vie, au culte de la vérité pure, je ne me suis jamais mêlé à la lutte des intérêts pratiques qui divisent les hommes. J'ai vécu dans mon laboratoire solitaire, entouré de quelques élèves, mes amis.'

When he won the Jecker Prize, he was in his thirty-fifth year. The appointment to the Chair at the Collège de France made it possible for him to marry Mademoiselle Bréguet, the daughter of a French Swiss, whose family had made money by manufacturing watches, famed since

the middle of last century. Monsieur Bréguet was a *constructeur industriel*, or builder of factories. He lived near the Place de l'Hôtel-de-Ville, on the Quai de l'Horloge, and the families were acquainted from early days. Mademoiselle was a desirable *partie*, well-dowered, and of great beauty, which she retained up to the end of her life. She was placid in manner, with lovely eyes, and a brilliant complexion, rendered even more striking, when, at an advanced age, her hair was silver; and in the church of Saint-Étienne du Mont there is a picture of Sainte-Hélène, the lovely face of which is taken from a portrait of Madame Berthelot as a girl. The meeting of the young couple was somewhat romantic; Mademoiselle Bréguet, no doubt, must have appeared to Marcellin to be beyond his reach, and besides, his attention was otherwise occupied. But one day, on the Pont Neuf, Mademoiselle was crossing the longest bridge in Paris in the face of a strong wind, wearing a charming Tuscan hat, then the *mode*. Behind her walked her future husband; suddenly she turned round, to avoid having her hat blown off, and practically ran into his arms. If not exactly love at first sight, it was a case of love at first touch. Their married life was of the happiest; indeed, it may be said that they were in love with each other till the end. One of the sons wrote: 'Mon père et ma mère s'adoraient; jamais le moindre nuage n'avait troublé leur bonheur. Ils s'étaient compris dès le premier jour. Ils étaient si bien faits pour se compléter! Bien que très lettrée et fort intelligente, maman s'était toujours effacée devant son mari, se bornant à s'efforcer de le rendre parfaitement heureux. C'était, à son avis, la seule façon de collaborer à son œuvre.' Another intimate friend added: 'Monsieur et Madame Berthelot s'adoraient; tous deux étaient de la nature d'élites; sa compagne n'avait cessé de l'encourager et de le soutenir.' No one visiting their house could fail to

remark this absolute devotion to each other; never was there a happier family. Although not a conversationalist, Madame Berthelot, by her perfect tact, her serene manner, and her charming sympathetic face, knew how to make each guest appear at his best; the ball of conversation was lightly tossed round the table, Berthelot himself, by his quaint and paradoxical remarks, contributing his share. A dinner at Berthelot's, in his old house in the Palais Mazarin, the home of the Institute, was a thing to be remembered. Always charitably disposed, Madame Berthelot used to send all the cast-off clothes of the family to the cleaners, and after they had been carefully mended, they were distributed to poor friends.

In 1881, Berthelot was elected a 'Permanent Senator'; he thought it incumbent on him to bear his share in the government of his country. With his wife's help, he managed to carry on his two functions at the same time. In his place in the Senate, Berthelot used to sit buried in his arm-chair, his head thrown back, and his eyes closed, apparently inattentive to all that passed; but nothing of importance escaped him. He took a leading and active part as member of various Committees dealing with education, and in 1886, as Minister of Education in the Goblet Cabinet, he busied himself with the reform of educational methods in such a manner as to acquire a wide popularity; the Bills introduced by him dealt with primary and with higher instruction, with universities, and with technical schools; in the last he was no believer, except in so far as manual training was given. Later, in 1895, he was for a short time Foreign Minister in the Bourgeois Cabinet; but the delays of parliamentary procedure were not to his mind. It was with difficulty that he was persuaded to sign the Anglo-French Treaty defining the position of Siam; and, almost immediately after, he resigned office.

Berthelot's career is easily told; it consisted of honour after honour. He was elected a Member of the Académie de Médecine in 1863, and in 1867 he collaborated in the foundation of the École des Hautes Études, and in the reorganisation of scientific teaching. Membership of the Académie des Sciences followed in 1873, and in 1889 he became its Sécrétaire Perpetuel.

In 1900, he had the rare honour of being elected among the immortal forty in the Académie Française, succeeding to the Chair of Joseph Bertrand. Of 28 voters, 19 voted for him, 9 abstaining. Four years later, in 1904, he delivered the statutory discourse. He was a Member of the *Conseil Supérieur des Beaux-Arts*, of the *Conseil Supérieur de l'Instruction Publique*, and in 1886 he was created a Grand Officier of the Legion of Honour. He was Foreign Member of almost every scientific society in the world, including our own Royal Society.

On November 24, 1901, the Berthelot jubilee celebration, the anniversary of his seventy-fifth birthday, was held in Paris, M. Loubet, President of the Republic, in the chair. It took place in the great hall of the Sorbonne; all the Cabinet, the ambassadors of all countries, and delegates from universities and scientific societies from all over the world were present. Madame Berthelot with her children and grandchildren occupied a conspicuous place, beaming over with unaffected pleasure; Berthelot had declined the State offer to make a triumphal procession in the carriage of the President with a military escort; he went on foot from the Quai Voltaire to the Sorbonne, his greatcoat buttoned so as to hide the grand-cordon of the Legion of Honour, and his head down so as to avoid recognition. He was embraced by the President of the Republic, and amid the enthusiastic applause of the spectators, address after address was delivered, each delegate conveying the congratulations of the body which he represented. It

was a national fête. Thus did the French honour science and its *doyen*.

On March 18, 1907, the end came. Madame Berthelot had been ailing for about three months; it turned out to be an attack of heart-disease, dangerous at the age of seventy. After she was confined to bed, Berthelot watched by her each night, seated in a deep arm-chair, only leaving her when she was asleep. He himself suffered from the same disease, and it was accelerated by his want of rest. His family noticed his feverish appearance in the mornings; he excused himself by saying that he was finishing a memoir for publication. On Passion Sunday there was a slight improvement, and Berthelot passed the afternoon in his laboratory at Meudon. That night, however, Madame Berthelot became comatose, and her husband never left her bedside until Monday at four, when the end came. Berthelot suddenly rose from the arm-chair in which he was seated, threw his arms in the air, uttered a cry, and fell back dead. They died, as they had lived, together.

It now remains to give a sketch of Berthelot's scientific work. The 'Prix-Jecker' has already been alluded to. This was the reward of his labours on the synthesis of carbon compounds. He began in 1851 by investigating the action of a red-heat on alcohol, acetic acid, naphthalene, and benzene; this led him in 1860 to the rediscovery of acetylene, a compound originally obtained by Edmund Davy, Sir Humphry's brother. In 1856 he synthesised methane by the action of a mixture of sulphuretted hydrogen with carbon disulphide on copper; and in 1862 he obtained ethylene and acetylene by heating marsh-gas to redness. His condensation of acetylene to benzene in 1866 established the first link between the fatty and the aromatic series. His direct synthesis of acetylene from carbon and hydrogen in 1862, and the formation of alcohol

by hydrolysing ethyl-sulphuric acid, obtained by absorbing ethylene in sulphuric acid, taken in conjunction with his synthesis of hydrocyanic acid in 1868, pointed the way to the formation from the elements of innumerable complicated compounds of carbon.

Much light has also been thrown by Berthelot on the alcohols. In 1857 he produced methyl alcohol from marsh-gas by chlorination and hydrolysis; in 1858 he recognised cholesterine, trehalose, meconine, and camphol as alcohols; in 1863 he added thymol, phenol, and cresol to the same class; and he showed how to diagnose alcohols by acetylation.

Turning to the esters, the nature of glycerine occupied his attention in 1853; in that year he succeeded in synthesising some animal fats, and showing their analogy with esters, as has already been mentioned; and he prepared other salts of glyceryl by submitting it to the action of acids. The action of hydriodic acid was, however, found to yield two substances of a different nature, namely isopropyl iodide, and allyl iodide; and from the latter he prepared, for the first time, artificial oil of mustard. The analogy of sugars with glycerine led him to investigate the action of acids on sugars, and this resulted in the synthesis of many of their esters. The fermentation of mannite and other polyhydric alcohols was also studied in 1856 and 1857, also the conversion of mannite and glycerine into sugars, properly so called. The esters of pinite, etc., with tartaric acid, were also studied, and in 1858, trehalose and melezitose were discovered. In 1859, Berthelot maintained that the action of yeast is not a vital, but a chemical phenomenon; and he returned again and again to the study of fermentation.

These and other similar investigations on esters led him, in conjunction with Péan de Saint-Gilles, to investigate the rate of esterification; and the experiments, begun in

1861, led to a long piece of work on chemical equilibrium, and on 'affinity.' In 1869 he attempted to limit the action of hydrochloric acid on zinc by pressure, but unsuccessfully; and in the same year he investigated the equilibrium between carbon and hydrogen, in sparking acetylene under pressure. And later in that year he announced laws, describing the partition of bodies between two solvents, and he investigated the state of equilibrium in solution. In the same year appeared the first of the long series of researches on thermal chemistry. In 1875 he returned to the subject of chemical equilibrium, dealing with the partition of acids between several bases in solution.

Among other syntheses was that of formic acid from caustic soda and carbon monoxide; oxalic acid was produced by the oxidation of acetylene; and acetates, by the slow oxidation of acetylene, in contact with air and caustic potash, in diffuse daylight.

In 1857 the combination of unsaturated hydrocarbons with the halogen acids was studied, as well as the conversion of chloro- and bromo-hydrocarbons into hydrocarbons by reduction. In 1860 ethyl iodide was synthesised by the union of ethylene with hydriodic acid; and in 1867 the use of a concentrated solution of hydriodic acid as a universal reducing agent at high temperatures was discovered.

Berthelot's numerous and important researches on the acetylides of silver and copper doubtless led him to pay attention to explosives. Begun in 1862, they were continued until 1866; and in that year he enunciated the theory that the production of mineral oils may conceivably have been due to the action of water and carbonic acid on acetylides of the alkaline metals, and to the subsequent resolutions of acetylene at a high temperature into other hydrocarbons. These researches on the acetylides were

followed in 1870 by investigations on the explosive force of powders, the explosions being carried out in a calorimeter.

In 1871 Berthelot proceeded to investigate the detonation of mixtures of gases, and he made measurements of the heat of formation of nitro-glycerine. In 1874 and 1876 the work was continued; and in 1877 it was extended to the temperatures of explosive mixtures, and to the velocity of combustion. In 1878 explosive mixtures of dust with air, and in 1880 fulminating mercury, were examined. A research on the velocity of the explosive wave in gases followed in 1882; and in 1884 measurements of the specific heats of gases at high temperatures were made. In the same year the calorimetric bomb was invented; and in 1892 it was adapted to the requirements of organic analysis.

Allotropic varieties of the elements also claimed Berthelot's attention. In 1857 he commenced with a study of allotropic varieties of sulphur; and in 1870 he investigated these varieties thermally. In 1869 he examined the allotropic varieties of carbon, and this led him to the preparation of various forms of graphitic oxides. Allotropic silver and other allotropic forms were also the subject of his research.

Berthelot also did much work by help of the 'silent discharge.' Attracted to it in 1876, when he submitted mixtures of organic substances with nitrogen to its influence, and succeeded in causing the nitrogen to enter into combination, he repeated Brodie's experiments, and reproduced the oxide C_4O_4. In 1878 he produced by the same means the higher oxide of sulphur, S_2O_7, in needles often a centimetre in length, and in 1881 pernitric anhydride. In 1895 he carried out similar work with argon, and later with helium.

From an early date Berthelot interested himself in

agricultural chemistry. From his laboratory at Meudon, assisted by his colleague, André, have appeared a succession of memoirs, chiefly relating to the absorption of nitrogen by plants, and to their behaviour under the influence of electric energy. To the very end his interest was kept up in these experiments; and he was hopeful of increasing by electrical means the productiveness of cereals, and of adding to the world's food-supply.

Though so keenly alive to the present, the past had for Berthelot a great attraction. In 1877 he analysed a sample of Roman wine, which had been preserved in a sealed flask; and he has contributed to the Journals many notices of the composition of ancient objects of metal. His works on *Les Origines de l'Alchimie*, and on a *Collection des anciens Alchimistes grecs, texte et traduction*, and his *Introduction à l'étude de la Chimie des Anciens et du moyen âge*,' involved long research of ancient manuscripts; he acquired facility in reading ancient Greek, though for Arabian sources he was dependent on others.

Berthelot was the author of numerous works besides those on Alchemy. In 1872 he published a Treatise on Organic Chemistry; a fourth edition appeared in 1899. This was followed by *La Synthèse chimique; Essai de Chimie méchanique* (1879), in which he announced the principle of 'maximum work,' a doctrine afterwards withdrawn, or, at least, greatly modified in 1894; *Traité pratique de Calorimétrie chimique* (1893); *Thermochimie: Données et lois numériques* (1898), in which an account of his long series of calorimetrical measurements is given; this work and that of Julius Thomsen on *Thermochimie* are the standard books on the subject, and each contains the results of the individual researches of its author.

Berthelot's mind was one which interested itself greatly, not merely with things, but with their origins; and in

Science et Philosophie and *Science et Morale* he treats of the relation of science to human thought. The same critical spirit manifests itself in his *Histoire des Sciences: La Chimie au moyen âge,* in which Syrian and Arabian Alchemy is treated of.

A partisan of Lavoisier, *La Révolution chimique de Lavoisier* presents that point of view strongly. He also published in 1898 his correspondence with Renan.

The lectures which he delivered at the Collège de France were published under the titles *Leçons sur les Méthodes générales de Synthèse en Chimie organique; Leçons sur la thermochimie; Leçons sur les principes sucrés;* and *Leçons sur l'isomérie.* The application of thermal chemistry to problems of life was treated of in his *Chaleur animale,* and in 1901 he published three volumes on *Les Carbures d'Hydrogène.*

One point remains to be mentioned. It has sometimes been objected that Berthelot kept science on a wrong path by persistently retaining the old system of representing formulæ, after all the rest of the world had abandoned it. The writer remembers well a conversation in the late '80's, in which Berthelot defended his views. He thought the position of those who employed the customary notation (and, of course, they comprised practically the whole chemical world) not unlike that of the defenders of the phlogiston theory! The retort was obvious, but not made. Berthelot had not even the excuse of Cavendish, who, after a calm, deliberate statement of the results of his research in terms of the then new hypothesis of Lavoisier, restated it in terms of the phlogistic method, saying that he preferred to make use of the older and better known language, rather than of the newer modes of expression. For in 1890 Berthelot was, perhaps, the only survivor of the older chemists. Professor Guye, who attended his lectures in 1890-91, tells that the session was

begun, as usual, with the special notation of which Berthelot was the sole defender ('equivalents based on two volumes of vapour'), and that, without the slightest warning in the middle of a *chapitre*, to the great astonishment of his audience, he effected the change, dealing with a subject of which the first portion had been expounded in the 'equivalent' notation, and continuing in the newer notation of which he had so long been the opponent.

No one is more conscious than the writer that he has failed to do justice to this remarkable personality. His only excuse is that he has done his best. He wishes that it were possible to convey to the reader a sense of the brilliancy, the vivacity, the power, the ability, the talent, and the high character of the great chemist. In the life-like plaquette by Chaplain, his features and his attitude have been admirably reproduced. Truly he was one of the most remarkable of the eminent men of whom France may be proud. He and his wife lie in the vaults of the Pantheon, in life united, in death not put asunder.

II. CHEMICAL ESSAYS

HOW DISCOVERIES ARE MADE

There is a difference between discovery and invention. A discovery brings to light what existed before, but what was not known; an invention is the contrivance of something that did not exist before. I suppose, however, that inventions and discoveries are made in much the same manner; though I have no claim to speak as an inventor, except in a very small way.

Many people, probably most people, think that when a discovery is made, it comes all in a flash, as it were—that a new idea suddenly crops up, and its conception is a discovery. That may sometimes be the case.

We have all heard of the puzzle given to Archimedes; how he was asked to find out, without injuring it in the least, whether a certain crown consisted of silver or of gold; and by weighing it in air and in water, he invented the method of taking specific gravity; for the crown when weighed in water lost weight equal to that of the water which it displaced. And he ran through the streets of Alexandria, crying, 'Heureka! I have found it!'

His finding that the crown was of gold was a *discovery*; but he *invented* the method of determining the density of solids. Indeed, discoverers must generally be inventors; though inventors are not necessarily discoverers.

It is too often supposed that, like the poet, discoverers are 'born, not made'; but I think I shall be able to show that many people, though not all, have in them the power of making discoveries; and if this short article can give to any one the hope of making discoveries, and prompt him to try, it will have more than achieved its object.

Like every other endeavour, the beginning is in small things. Any one who tries to look into anything with sufficient care will find something new. A drop of water; a grain of sand; an insect; a blade of grass; we know indeed little about them when all is told. First, of course, we must learn what others have done; and for that purpose we go to school and to college, and read books and hear lectures. Before beginning, we should at least have an idea of what has been achieved by our predecessors. After that there is nothing for it but to try.

But there are two ways of trying: and what I wish to convey is best told in an allegory.

There are two kinds of fishers: those who fish for sprats and those who angle for salmon. I do not say there are not others; but these two kinds are at the extremes of the fishing world. The fishers for sprats are sure of a large catch, or at least of catching something; but the fish are small, not particularly attractive as food, and of no great value; they are, however, numerous and easily caught. But the salmon fisher hunts a very different prey; if there is any special quality about a salmon, it is his power of motion and his fickleness of taste; so that the angler, when he casts his line, is by no means sure that a fish is within reach of his cast, nor, if he is, whether he will rise to the fly. If fate is propitious, however, his prize is a great one; his pleasure consists not merely in catching the fish, but in struggling with him, possibly for an hour or more; wading after him in alternate hope and fear—hope that his line may stand the

strain, fear that it may part, or that some hasty movement may lose him his fish.

Most discoverers are like fishers for sprats: they go where they are sure of a reward; but the gain is not great, at least as regards sport. It is much more fun to fish for salmon; but then there is a great chance that the angler has mistaken the place to fish, or that he has used the wrong fly; or that the weather is unfavourable; or that a hundred things, impossible to foresee, will prevent the salmon taking the hook.

We may not pursue the allegory further: salmon are now not nearly so plentiful at they used to be; sprats, perhaps even more numerous. And it requires training and a good eye to know where the salmon lie and in what pools to fish.

But let us dismiss this image and become historical.

One of the first puzzles which awaited solution was the nature of flame. The ancients believed it to be an element—that is, a property, or perhaps a constituent of most, or of all, other things. Flame, said they, is hot; and everything which is hot partakes of the nature of flame.

Robert Boyle guessed that it was a sign of the rapid movement of the minute particles of which he supposed everything to be composed; but this, although very near what we now suppose to be the truth, was merely a lucky guess; for he had no real ground for making the suggestion. It was noticed that flame appears when anything burns; and the reason for combustion, or burning, had first to be sought.

The real step towards this was made by Joseph Priestley, an English dissenting minister, and by Karl Scheele, a Swedish apothecary, almost at the same time. Priestley was a fisher for salmon, to revert to our old image; he fished everywhere and caught many large fish. And so

was Scheele. They noticed that when certain substances were heated, gases—or, as they termed them, 'airs'—escape. For it had been supposed that all gases, as we now name them, were merely modifications of ordinary air; just as we sometimes notice a pleasant or a disagreeable smell, and attribute it to the 'goodness' or 'badness' of the air, so it was generally thought that gases, such as coal-gas, were a sort of air with an unpleasant odour and the curious property of catching fire.

About fifteen years before Priestley and Scheele made their great discovery of oxygen, the constituent of air which supports combustion, a Scottish professor, Joseph Black, investigated the particular kind of 'air' which escapes when chalk or limestone is heated. And he made the great discovery that this 'air' can be reabsorbed by lime—the residue left after chalk is heated—so that chalk is again formed.

Moreover, he weighed the chalk before it was heated, he measured the gas, and he weighed the lime left after the gas had been driven off from the chalk. And lastly he weighed the chalk which was re-formed after the lime had absorbed the gas.

He found that the lime was lighter by just as much as the gas weighed; and he called this gas 'fixed air,' to emphasise the fact that it could be 'fixed' or absorbed by lime and similar substances.

This first opened the way for the investigation of gases; it was a great discovery—perhaps one of the most fertile which has ever been made. It is to be noted that Black was not content with this, however; for he recognised that the fixed air from chalk was of the same nature as steam from water. And just as it was necessary to heat water so as to drive it into steam, so it appeared to him that carbonic acid gas, to give 'fixed air' a more modern name, was a gas by virtue of the heat or 'caloric' which it

contained. He went on to discover how much heat is required to convert a known weight of water into steam. He found that about fifty-four times as much heat is required as is necessary to heat the same weight of water from the freezing-point to the boiling-point. But the steam is no hotter than the boiling water; hence Black called this heat the 'latent heat' of steam, because it lies hidden in the steam and does not affect a thermometer. Black made *quantitative* experiments — that is, he not merely made discoveries, but found the *quantities* in which the changes took place.

The way was now plain for Priestley and Scheele. They heated all kinds of substances: if they evolved gas, that gas was collected and examined; but neither Priestley nor Scheele paid much attention to quantities. The methods of dealing with gases had to be *invented,* moreover. And while Scheele caught his gases in bladders, Priestley invented, or rather reinvented, what he called a 'pneumatic trough,' a vessel filled with water containing jars and bottles standing inverted full of water. If the tube leading from the retort in which the substance evolving the gas was heated was directed so that its open end was directly under the mouth of the bottle, the escaping gas entered the bottle and displaced the water; and when the bottle was full, it could be corked, still under water, and removed so that the gas could be examined.

It is usually the case that discoveries have to be accompanied by inventions; the sequence is that to try any new thing, a piece of apparatus has to be devised which will effect the purpose—or perhaps an apparatus already known has to be altered—so that it may almost be said that invention and discovery go hand in hand.

For this reason it is very important that the discoverer should be a good worker in all kinds of materials—in glass, for most small pieces of apparatus can best be constructed

of glass: in brass, for if anything of the nature of machinery such as pumps, stirrers, etc., is required, brass is perhaps the most convenient material; in clay, for vessels are wanted which will withstand a high temperature; and of recent years silica glass, made from fused rock-crystal, has proved of great use, for it can be worked before a blow-pipe fed with coal-gas and oxygen.

But to return to the discovery of oxygen. Priestley heated oxide of mercury, or, as he called it, 'red precipitate,' in a retort, and collected the escaping gas; and he found that a candle burned in it much more brightly than in air; and, moreover, after having found that a mouse could live in it longer than in the same volume of air, confined in a bottle, he breathed it himself and found that its effect was pleasant and exhilarating.

Similar experiments were made by Scheele with the same result; but Scheele went much further. Having noticed that a number of substances had the property of making combustible bodies, such as wood, flour, and charcoal, deflagrate, or burn more brilliantly when mixed with them, he heated these substances, and found that they too evolved oxygen gas. Among the substances were red-lead, black oxide of manganese, nitre, and many others; so he established a general rule that those substances which can be mixed with charcoal to make a kind of gunpowder will evolve oxygen when heated.

It thus became known that air contained a gas, amounting to about a fifth—Scheele says a sixth—of its bulk, possessing the property of making combustible objects burn with greater vigour. Flame, therefore, was caused by the action of oxygen, as the new gas was called later, with combustible bodies.

It would take too long to consider the curious doctrine of 'phlogiston,' an immaterial effluvium which was supposed to escape when bodies burn; I can merely mention

that Lavoisier, a celebrated French chemist, gave the correct explanation of combustion—namely, that it is caused by the union of oxygen with the substance burning. Lavoisier, however, cannot be ranked as a great discoverer, though he shone as an interpreter of the discoveries of others.

Henry Cavendish, who did his best work between 1770 and 1790, discovered the composition of water; that it is produced when oxygen and hydrogen unite; and he determined with great accuracy the proportions by volume in which the union of the two gases is completed. He also attempted to show, by passing electric sparks through a mixture of the inert gas of the atmosphere, nitrogen, mixed with oxygen, that nitrogen was a single substance and not a mixture; nearly all the nitrogen disappeared under this treatment, only about one hundred-and-twenty-fifth of the whole being left. It would hardly have been possible for him, in the existing state of knowledge, with the imperfect appliances which alone were available at that time, to have identified his inactive residue with 'argon,' a gas discovered more than a century later; for the spectroscope was then unknown, and it is the chief means of identifying and characterising gases, and indeed elements of every kind. This is an example of how discovery has sometimes to wait on invention; for, until the instruments of research are invented, it is almost impossible to confirm a discovery, even although it may be genuine.

The true nature of flame, which, as before remarked, has been a puzzle since the remotest ages, has had to wait on invention for its discovery. When a current of electricity of high tension, such as is produced by an induction-coil or by an electric machine, is passed through any rarefied gas, it gives out a peculiar and often a very beautiful coloured light: sometimes red, as in the case of

hydrogen or neon; sometimes bluish-white, as with carbonic acid or krypton; sometimes purple-red, as with argon or nitrogen. When examined through a prism or a spectroscope, this light is seen to consist of a number of colours, which blend to give the colour seen with the naked eye.

Thus the brilliantly red spectrum of hydrogen is easily shown to be a compound impression; the red light, which is the brightest, is mixed with and slightly modified by a blue-green and a violet light. Tubes which are well adapted to show this light were invented by a German physicist named Plücker in the 'fifties. Twenty-five years later, Sir William Crookes, with the aid of his skilful assistant, Mr. Gimingham, improved the then existing form of air-pump, invented by Dr. Hermann Sprengel, so that it became capable of exhausting the air much more completely than was previously possible.

He found that, at a much greater exhaustion than that which causes gases to glow and give out their spectrum, a current of high-tension electricity produced in the tube a violet or a green phosphorescence, according as the glass of which it was made contained lead and potash, or lime and soda, combined with the silica, or sand.

Moreover, the position of this curious phosphorescent glow depended on the shape and direction of the wire or plate from which the negative electricity passed into the tube. From a wire the glow proceeded in all directions perpendicular with its length, so as to colour the tubes immediately surrounding the wire with phosphorescent light. If the wire, however, were terminated with a plate, then the phosphorescent light appeared mostly between the front of the plate and the positive wire of the vacuum-tube. Supposing the plate were curved, so as to form a concave metallic reflector, the light of what was evidently

a discharge was concentrated on a point at the focus of the metallic mirror.

Moreover, if an object of any kind were placed at the focus, and submitted to the discharge, it became intensely hot; or if it could move—if, for instance, it formed the vanes of a little wheel or windmill—the wheel revolved rapidly as if it were being bombarded by infinitesimally small bullets. Crookes imagined that by being thus highly rarefied, the gaseous matter changed so as to become 'ultra-gaseous,' that it changed its state in somewhat the same manner as ice becomes water or as water becomes steam.

It is interesting here to recall how Sir William Crookes came to make these most remarkable discoveries. He began by using a spectroscope to investigate the spectrum or coloured light given out by the various constituents into which he had analysed the dust which deposits in the flues used to convey the sulphurous acid produced by the burning of pyrites (a compound of sulphur and iron), then (in the 'sixties) recently introduced as a source of sulphur for the manufacture of sulphuric acid or oil of vitriol. One of his precipitates, when examined with the spectroscope, showed the presence of a bright green light; and this was traced to the presence of a new element, to which he gave the name 'thallium,' from the Greek *thallos*, a green twig.

One of the first things done with a new element is to try to discover its 'equivalent'—that is, the proportion by weight of the element which will combine with 8 parts by weight of oxygen. (The number 8 is chosen, because 8 parts by weight of oxygen combine with 1 part of hydrogen to form water.) The weighings require to be very accurately made; and a peculiarity which affects all attempts to weigh very accurately must now be told of. The question is often asked as a catch:—'Which weighs

most: a pound of feathers or a pound of lead?' The usual answer is, 'They weigh the same.'

Although this is strictly true (for a pound is a pound, whether of lead or feathers), a little consideration will show that when the feathers are placed on one pan of a pair of scales and the lead on the other, the lead takes up far less room than the feathers; in other words, the feathers displace much air, while the lead displaces little. That is, the air which the feathers displace no longer rests on the pan; and if it were still there, the feathers would weigh more. Hence a so-called pound of feathers weighs less than it ought to by the weight of the air displaced.

Now to overcome this difficulty and to avoid the somewhat complicated and uncertain calculations necessary to ascertain the true weight of the things weighed, Sir William Crookes devised a balance closed in by a case in which a vacuum could be made. And it was while obtaining this vacuum that he discovered that light apparently (but really heat) appears to repel certain objects more than others. Thus he was led to experiment on vacuum-tubes and to perform all the beautiful experiments which have made his name so famous. At the same time he *invented* the 'radiometer,' a pretty little toy for showing the repelling action of heat.

Here again we see the advantage of following up small trails; they may widen to great and most important roads. If Sir William had been content to weigh his compounds of thallium in his vacuum-balance, as most others would have done, and had not had the genius to follow this side-track, he would have missed many of his greatest discoveries.

A further great step was made when the German physicist Lenard found that Crookes's 'rays'—the 'fourth form of matter' which he supposed to be repelled from

the negative pole of the Plücker tube when very highly exhausted—could pass out of the tube through a thin 'window' of the very light and strong metal aluminium. It is true they could not pass very far; they soon became scattered. Here was a discovery made with a set purpose. Professor Lenard wished to decide the question whether Crookes's 'rays' were really due to a stream of corpuscles or whether they were vibrations like those of light.

Sir William had previously found that if a magnet were placed near the tube the path of the rays was no longer straight, but curved. And Lenard observed that if the aluminium window were placed so that a 'vacuum' (not a complete, but a nearly complete one) were on both sides of the aluminium window, the 'rays' could be bent out of their course by the magnet after passing through the window.

It must be remembered that these rays are not themselves visible; it is only possible to see where they strike by their causing phosphorescence. Professor Röntgen, the celebrated German physicist, discovered in his turn that if these rays be suddenly stopped—say by falling on glass or metal—rays of another kind are sent on, which have the power of affecting a photographic plate and of rendering certain substances exposed to them phosphorescent; so that, as different kinds of matter have very different powers of stopping Röntgen rays, it is possible to photograph the bones of the body, although the flesh is comparatively transparent to them. The bones, as it were, cast their shadow; or the shadow of the bones can be thrown on a piece of card, painted with material which phosphoresces and shines when exposed to the impact of the rays.

I believe that Röntgen's discovery arose from an accidental observation that a box of photographic plates left near a Crookes's tube became 'fogged,' and he too had genius to follow up this clue.

We are getting on rather slowly, however, in the hunt for an explanation of flame. A great step in advance was made by the discovery of radium by Madame Curie.

Radium is a metal, the salts of which continually give out 'Lenard rays,' or 'Crookes's rays.' And it is certain that it is losing substance during their emission.

Mr. Soddy and I have actually trapped and measured one of the products which is being thrown off by radium while these rays are being shot out; it is a gas called 'radium emanation.' And it in its turn decomposes and is changed to some extent into the gaseous element helium, which I discovered in 1895.

All the while that these changes are taking place, what are called 'β-rays' (beta-rays) are being evolved, and the opinion is now generally held that these so-called rays are really negative electricity, and are identical with the 'cathode-rays' of Lenard.

I have been frequently asked: 'But is not electricity a vibration? How can wireless telegraphy be explained by the passage of little particles or corpuscles?' The answer is 'Electricity is a *thing*; it *is* these minute corpuscles, but when they leave any object, a wave, like a wave of light, spreads through the ether, and this wave is used for wireless telegraphy.'

It has been found that flames are capable of conducting electricity, while gases, under the usual atmospheric pressure, are very good insulators, and sparks can pass through air only when the current is one of very high tension. Now, in flames rapid chemical action is taking place; compounds are burning—that is, their constituents are in the act of uniting with oxygen.

Although it is not certain that β-rays—or, to give their other name, corpuscles of electricity—are being shot out during such changes, it is not improbable that they are. No doubt they impinge on the neighbouring atoms and

set them in rapid vibration; and they may even break up molecules and cause them to assume other forms of combination. And in doing so, very short electric waves are sent out through the ether, and these are what we term 'light,' and 'radiant' heat.

There are several other lines of evidence which support this notion. For example, a pure gas cannot be heated red-hot or made to glow by heat alone. There must be a chemical change of some kind at the same time. Again, a Welsbach incandescent gas mantle, if made of pure thoria (and that means 'nearly pure,' because we are not acquainted with really pure substances), does not give out much light when heated, but if some other earth, such as oxide of cerium, is mixed with the thoria, the familiar brilliant incandescence is produced when it is heated by a Bunsen burner. The 'pencil' of a Nernst lamp is made chiefly of zirconia, another earth; and here, again, unless the zirconia is mixed with a trace of some other oxide, it will not glow very brightly when a current of electricity is passed through it. In all these cases there is almost certainly chemical change, and also, no doubt, evolution of corpuscles of electricity which set the ether vibrating and so produce light and heat.

It may be asked: 'Do substances not lose weight when corpuscles are being shot out?' Professor Landolt, of Berlin, has been making experiments on the gain or loss of weight when a weighed quantity of substances capable of chemical change are mixed in a closed vessel; and he finds that in many cases there is a minute loss of weight. Perhaps that is due to the escape of corpuscles; but too few experiments have been made to allow of a definite answer.[1]

Perhaps, too, the corpuscles when expelled are not moving very rapidly, and are thus absorbed by the sides of the vessel in which the reaction takes place; and this

[1] He has since shown that there is no change of weight.

may also be the case with flames. A flame, however, if brought near an object containing an electric discharge will discharge it; and this may possibly be due to the action of electric corpuscles on the charged object.

It will be seen, then, that we do not know yet with certainty what flame is, but we are getting on the track. And the direction in which to make experiments is clear. Whosoever asks shall receive, but he must ask sensible questions in definite order, so that the answer to the first suggests a second, and the reply to the second suggests a third, and so on. If that course be followed, it will certainly result in discoveries, many of which may be important and lead to inventions of great practical value. For, indeed, an invention is often definable as a method for utilising a discovery.

THE BECQUEREL RAYS[1]

It is remarkable how the writings of ancient authors often contain a forecast of subsequent discoveries. Puck's projected girdle round the earth, which was promised completion in forty minutes, has been surpassed many hundred times by the rate of the electric current in a telegraph-wire; and Robert Boyle's suggestions regarding the nature of the air are on the high-road towards verification. He wrote, about the year 1670: 'Our atmosphere, in my opinion, consists not wholly of purer æther, or subtile matter which is diffused thro' the universe, but in great number of numberless exhalations of the terraqueous globe; and the various materials which go to compose it, with perhaps some substantial emanations from the celestial bodies, make up together, not a bare indetermined feculancy, but a confused aggregate of different effluvia.'

Up to 1894, it was supposed that our atmosphere consisted mainly of the two gases, nitrogen and oxygen, together with minute quantities of carbonic acid, water-vapour, ammonia, peroxide of hydrogen, and ozone; but in that year it was shown to contain a not inconsiderable amount of an inactive gas, argon; and crude argon has since been found to contain minute quantities of no fewer than four other similar gases. Small traces of hydrogen have also been discovered in air; although a large percentage of

[1] An article which appeared in the *Contemporary Review*, 1902.

hydrogen would render air explosive (for water is formed with explosive violence when hydrogen and oxygen combine), yet traces of hydrogen may coexist with oxygen without combination, except when the mixture is actually in contact with a flame.

These, however, are not the only constituents of the atmosphere; and in the following pages an account will be given of certain phenomena which render it exceedingly probable that still more 'subtile matter' is on the eve of discovery.

In order to follow the course of events, it is first necessary to devote some attention to the supposed nature of light. Owing to its being perceived by our special organ of sense, the eye, it early attracted attention. At first believed to consist of corpuscles, shot out from the luminous body, it is now recognised as arising from the vibrations of a medium pervading all space, termed *ether*; and the propagation of light takes place much as waves spread in a pond, except in this: the particles of ether, unlike the waves of water, are not restricted in their motion to one plane, but the oscillations may take place in all directions at right angles to the direction of propagation. There appears, however, to be no limit to the mode or magnitude of the ethereal waves; and though it cannot be positively stated that the wave-motion ever takes place, like sound waves, in the direction of propagation, still that mode of propagation of waves is not excluded. It is certain, however, that such a mode of transmission does not correspond with the nature of light, which consists wholly of transverse vibrations.

Just as it is possible to measure the distance between the crests of the waves of the sea, so it is possible to determine the distance between the crests of the waves of light, or in other words, to measure the wave-length of light. And it has been discovered that all the visible rays of

light are comprised in less than an octave; that is, the longest visible waves are not twice as long as the shortest visible. Their length, moreover, is not inconceivably minute. The twenty-fifth part of an inch, or a millimetre, although a small distance, is easily seen with the naked eye; indeed, the twentieth part of that length can still be estimated without the aid of a lens. The average length of a light-wave is about the hundredth part of that distance, or about the two-thousandth of a millimetre. The thousandth part of a millimetre is termed a micron, the symbol for which is the Greek letter μ; the wave-length of deep red light is $\frac{4}{5}\mu$, and of violet light $\frac{2}{5}\mu$.

There are, however, ethereal waves which cannot be seen. Those of greater wave-length give rise to the sensation of heat; they are termed 'infra-red' waves; while those of shorter period are accessible to photography, for they change the nature of the compounds of silver which form the sensitive coating of a photographic plate, and can thus be recognised; they are termed 'ultra-violet' waves. One of the difficulties of tracing the existence of the short wave-lengths by photography consists in the absorptive power which glass and air have for such waves. A pane of glass, though transparent to ordinary light waves, is nearly opaque to ultra-violet waves. Quartz or crystal, of which spectacle-lenses are generally made, is much more transparent to vibrations of short wave-lengths; but even quartz has its limits. By an ingenious contrivance for exposing a sensitive plate in a vacuum, so that the absorption of the air did not influence the result, Schumann succeeded in chronicling the existence of waves only $\frac{1}{10}\mu$ in length. On the other hand, Langley, by means of an exceedingly delicate apparatus for detecting heat-vibrations, termed a bolometer, has detected waves as long as 30μ. Between that wave-length and one two hundred times as great, six millimetres, there is a gap in

our knowledge; the longer waves are the vibrations th discovery of which was due to Hertz, which are produce by electric oscillations, and which are now being utilise for telegraphy without wires.

It is, however, with the shorter, and not with the longe vibrations, that we have to do. These are not incom mensurable with the dimensions of a molecule, for th larger molecules are believed to be about the milliont of a millimetre in diameter, or about one hundredth c the shortest wave-length which has been measured. An just as an interposed grating offers little opposition to th course of a large wave of water, while it will stop ripple so matter is sufficiently fine-grained not to oppose th spread of a Hertzian wave of great wave-length, althoug it may stop light and other vibrations of shorter wave length. It is known, indeed, that the signals of wireles telegraphy are not blocked by material obstacles such a houses, or even hills, while a very thin slice of brick o stone is opaque to light.

When two thin strips of gold-leaf are suspended from glass support, and given an electric charge, they diverge owing to the repulsive force between the charges of elec tricity which they contain. They will remain apart fo an indefinite time, provided the charge cannot escap through the support. But on exposure to ultra-viole rays, the electroscope, if charged with negative electricity is at once discharged, and the leaves fall together. Th electricity finds some means of leaving the leaves of gol and they drop, under the action of gravity. The rays d not, however, discharge a positively charged electroscop This is one of the most characteristic properties of th ultra-violet rays, and, as will shortly be seen, of rays fro sources other than luminous bodies. This fact was dis covered by Hertz.

The Becquerel family has contributed much to ou

knowledge of the phenomena of radiation, and furnishes as conspicuous an example as the Herschels of the heredity of a scientific faculty. Antoine Charles, born in 1788, was famous for his electric researches; Edmond, his son, born in 1820, was author, with his father, of a treatise on *Electricity and Magnetism*, and investigated the phenomenon of phosphorescence, of which more anon; and Henri Becquerel, born in 1852, while engaged in extending his father's work, made the wonderful discovery of the emission of rays from certain minerals containing the rare metals uranium and thorium. We must first, however, consider Edmond Becquerel's work.

Certain substances, after illumination, do not at once cease to give back light, but continue to glow, even after the source of light has been removed. Such substances, one of the best known of which is fluor-spar, are called *phosphorescent*. Some years ago, an attempt was made to utilise one of such substances—a carbonate of lime containing traces of sulphide of manganese—under the name of 'luminous paint.' Another class of substances has the property of transforming the vibrations they receive when illuminated into vibrations of a different period; among these are extract of horse-chestnut, and many artificial colouring-matters, one of the most striking being the lovely pink dye, eosin. Such bodies do not continue to emit light after excitation; they were differentiated from the former by Edmond Becquerel, and are termed fluorescent. The tendency of these substances is to convert short wave-lengths into longer ones. Thus a solution of acid sulphate of quinine, which is perfectly colourless by transmitted light, is opaque to ultra-violet rays. It reflects them, and at the same time increases the wave-length; hence by reflected light the solution appears to possess a violet shimmer.

It was in 1838 that Faraday investigated the luminous

appearance which accompanies the passage of a high-tension electric current through rarefied gases. Each gas gives out a soft, coloured light, totally different from the lightning-like sparks which pass between the positive and negative poles through gases at ordinary atmospheric pressure. The pressure must be reduced to about one-hundredth of its normal amount before such phenomena begin to appear; but the actual reduction of pressure depends on the particular gas submitted to the discharge. Under such conditions hydrogen glows with a red light; air with a pale violet glow; and carbonic acid has a steel blue appearance. The resistance of such rarefied gases to the passage of the electric current is much less than of gases at atmospheric pressure. As with solid conductors, it depends on the distance between the poles and the particular kind of matter employed. Hittorf, the eminent electrician, Professor in Münster, was the first to conduct experiments at still lower pressures, on still more rarefied gases, and he noted an increase in the resistance of the gas to the passage of electricity. Further, he observed that from the negative electrode, or cathode, the glow proceeded in straight lines, so as to cast the shadow of an interposed object on the opposite wall of the tube. He discovered, too, that such rays can be deviated by a magnet, a discovery made for the electric arc by Sir Humphry Davy in 1821. Sir William, then Mr., Crookes took up this subject in 1878, simultaneously with M. Goldstein, and has made it popular, by reason of the ingenious experiments which he devised to exhibit the rectilinear course of these rays. He devised a theory, moreover, to account for the rectilinear path, namely, that the electric current when it leaves the negative pole attaches itself to the molecules of gas, which, projected with great velocity, will pursue a parallel path, if the cathode is a flat piece of metal, or can

be focussed to a point, if the cathode be given the form of a concave mirror. Objects placed in the focus of such a mirror are bombarded, according to Sir William, and may be heated to whiteness by the impacts they receive from the prodigious number of moving molecules. Goldstein, on the other hand, conceived the phenomena to be due to a transmission of energy, apart from the conveyance of material particles; but he gave no precise definition of the nature of this transmitted energy. In 1883, however, Professor Wiedemann of Leipzig made the suggestion that possibly such 'cathode rays,' as the rectilinear discharges have since been termed, are composed of radiations of very short wave-length, shorter even than those of the most ultra-violet light. The same conception was held by Lenard. But while the cathode rays are deviated by a magnet, light waves are uninfluenced; and this forms an argument in favour of the former being due to projected particles. The suggestion has also been made, but on no sufficient grounds, that these phenomena are attributable to a longitudinal vibration of the ether, the waves being thus analogous to sound-waves in air—alternate condensations and rarefactions; or to choose a visible analogy, the longitudinal vibrations of a spiral spring, in which the coils periodically come closer together at one point of space, and then recede and become wider apart. A fourth hypothesis, similar to, yet differing from that of Crookes, is held by Professor J. J. Thomson of the Cavendish Laboratory, Cambridge. His view, which appears to be well supported by experimental evidence, is that each molecule of gas, in absorbing its electric charge, dissociates, or splits up, into two or more charged atoms or groups of atoms. Such charged portions of matter have long been taken for granted as existing during the passage of an electric current through a conducting liquid, and were named by Faraday, *ions*, or

travellers. An important argument in favour of this contention is, that the heat developed in such tubes is proportional to the intensity of the current, and not to the square of the intensity, as would be the case were the passage of electricity one of ordinary conduction. Thomson attributes the heat to the recombination of the ions to molecules, after discharge; and the number of ions would obviously be proportional to the intensity of the current and not to its square.

Goldstein and Crookes both thought that ordinary matter, such as glass or metal, was opaque to such cathode-discharges; but Lenard, following a suggestion of Hertz's, carried out at Bonn a beautiful series of experiments, which showed that the cathode rays could pass through a thin piece of aluminium foil, and be prolonged outside of the exhausted tube. Not only could they pass through ordinary air, although not to such a distance as through rarefied gases, but they also passed through a vacuum as perfect as could be produced by a mercury-pump, aided by intense cold to condense mercury-vapour out of the empty space. It appeared that the absorbing power of different gases is proportional to their specific mass.

The velocity of propagation of such cathode rays has been measured by an ingenious process by Professor J. J. Thomson. It is found to be approximately 200 kilometres, or about 124 miles a second. This, however, is enormously less than the velocity of light or of electric waves through the ether, which approximates to 180,000 miles a second. The accidental discovery by Professor Röntgen in 1896 that some flakes of platinocyanide of barium, placed near a Hittorf tube which was wrapped up in black paper, emitted a phosphorescent light, led to a great development of the subject. It was soon discovered that even behind a book of 1000 pages, or a plate of

aluminium half an inch thick, or a wooden board, luminescence was still produced. Röntgen investigated the transparency of various objects, and soon discovered that while skin and flesh are nearly transparent to these radiations, bone is comparatively opaque, and may be made to throw its shadow on a photographic plate or on a screen covered by phosphorescent material. The surgical bearing of this discovery was at once evident; and by help of 'skiographs' or shadow-writing the presence of a bullet embedded in the tissue can be recognised, and its exact situation localised; and in cases of fractures of bones, their exact shape can be made out, and they can be successfully set, for it is always possible to examine the position of the fractured ends through envelopes of bandages, which themselves are nearly transparent to Röntgen or X-rays.

One of the most remarkable properties of these rays is that they cannot be refracted by passage through a prism, nor apparently reflected from any object, however smooth and well-polished, nor can they be polarised. They are, however, absorbed by different substances unequally, and apparently the denser the substance the greater its absorbing power.

It might be supposed at first blush that the X-rays of Röntgen were identical with cathode rays. But if this were the case the X-rays should pass straight from the cathode through the walls of the tube, and proceed in a straight line; as a matter of fact, their point of origin can be displaced with a magnet, and if a spherical bulb be used to contain the cathode, each point on the bulb is a centre of emission, sending its radiations in all directions. Now Lenard had recognised that cathode rays could be differentiated into two distinct kinds. Suppose that they were made to pass through a hole in a block of lead, and to impinge on a photographic plate, if a magnet were placed

at one side, not only was there the image of a circle exactly opposite the hole, but also, at some distance from the circular spot, a diffused drawn-out impression, as if some of the rays had been unequally deviated by the magnet, and had impressed the plate separately. It is therefore probable that cathode rays contain some X-rays.

The wave-length of light can be measured by reflection from a metal plate on which from 14,000 to 25,000 parallel lines are ruled in each inch; such a prepared plate is termed a 'grating'; the modern gratings, which are wonderfully accurately ruled, are made by Mr. Brashier of Alleghany, New York State, by means of apparatus devised by the late Professor Rowland of Baltimore. Careful measurements by M. Perrin have proved that if the X-rays are due to ethereal vibrations at all, these cannot possess a wave-length greater than 0·04 μ, that is, less than half the shortest-waved ultra-violet vibrations which have ever been photographed.

Again, when light is passed through a slice cut from a crystal of tourmaline it is said to be polarised; it can pass through a second plate of tourmaline if held in a particular position, but if the second plate be rotated so that its second position is at right angles to its first, the light is cut off, and fails to pass through the second plate. M. Becquerel found that X-rays cannot be polarised; they pass easily through plates of tourmaline in whatever position relatively to each other they be placed. On the other hand, the rays emitted by phosphorescent bodies, which may be termed the Becquerel rays, are capable of polarisation. Hence they cannot be identical with X- or with cathode rays.

Lastly, it will be remembered that ultra-violet rays discharge negatively electrified bodies; they are without rapid action on bodies possessing a positive charge. But

X-rays discharge electrified bodies equally well, whether they be charged positively or negatively.

There is accordingly a certain degree of probability in favour of the view that cathode rays are due to molecular or ionic bombardment; but they are generally mixed with X-rays, which are apparently independent of matter for their propagation, and are therefore to be considered as due to disturbances of the ether. Ultra-violet rays, on the other hand, must be ethereal waves of very short wave-length; but they have the power of splitting gaseous molecules into charged atoms or groups of atoms, termed ions. It may be calculated, too, that the atoms conveying cathode-rays have a velocity of 124 miles a second; it would follow that of such atoms a single gram, or about one-thirtieth of an ounce, must have the same energy as a locomotive of 80 tons weight rushing at the rate of 50 miles an hour! No wonder, then, that they penetrate thin sheets of metal and embed themselves in glass.

In 1896 M. Poincaré, the well-known mathematician, suggested that all fluorescent substances might emit Röntgen rays; being guided to this guess by the hypothesis that it is the glass, against which the Röntgen rays strike, which phosphoresces and emits the rays. This suggestion was almost at once verified by M. Charles Henry, when he discovered that sulphide of zinc, a substance which shows marked phosphorescence, greatly increases the effect of X-rays when placed in their path. M. Henri Becquerel, too, in the same year, found that rays were emitted from a compound of the metal uranium, which affected a photographic plate wrapped in black paper, sufficient to exclude rays of direct sunlight. This power to affect a sensitised plate persists long after all visible phosphorescence ceases. Moreover, it is unnecessary to expose uranium salts to light before they are capable of producing a photographic image, for these

compounds may be prepared in the dark and still possess actinic power. And the rays emitted from them have the power of discharging both positively and negatively electrified bodies. Not merely salts of uranium possess this property, but even metallic uranium itself, a dark-coloured, brittle metal, which emits sparks of fire when shaken in a bottle, a phenomenon due probably to oxidation.

Shortly after this discovery of Becquerel's, Madame Curie, a Polish lady working in Paris, discovered that a certain specimen of pitchblende, the common ore of uranium, possesses the properties of uranium, and in greater measure. Pitchblende, though consisting mainly of an oxide of uranium of the formula U_3O_8, contains small amounts of other elements. On separating these, Monsieur and Madame Curie found that the bismuth obtained from this source is particularly radio-active, while ordinary bismuth shows no trace of that property. Attributing this behaviour to its containing a new element, they patriotically named it 'polonium,' in allusion to Madame Curie's nationality. But it was not long before they discovered that it was not only the bismuth which exhibited radio-activity, but also the barium; and they inferred the presence of a second element, naming it 'radium.' A third substance has also been separated from the same uranium ores by Debierne, who, following precedent, has termed it 'actinium.' It appears to be associated with another element, titanium, contained in pitchblende; and thorium, an element whose compounds were discovered to possess radio-activity by G. C. Schmidt, must be added to the list. We have therefore at present no fewer than four radio-active substances: polonium, associated with bismuth; radium, with barium; actinium, with titanium; and thorium. Associated with thorium is a much more powerfully radio-active material, to which

the name radio-thorium was applied by its discoverer, Otto Hahn.

Besides the properties already mentioned, radium, and presumably the others, have the curious property of changing a spark-discharge from an electric machine or a Ruhmkorff's coil into a violet glow-discharge; the interposition of a piece of lead, however, re-establishes the spark-discharge; and if barium bromide containing radium be held on the forehead between the closed eyes in a dark room, a distinct luminous haze is visible after a few seconds. The actinium rays, indeed, are said to be 100,000 times as powerful as those of uranium. Very powerfully radio-active preparations of barium chloride and bromide are now manufactured by various firms by processes devised by Madame Curie and by Professor Giesel.

A new light has been thrown on all these phenomena by Professor Rutherford, who has found that thorium compounds give out an 'emanation,' which may be likened to one of Boyle's 'exhalations of the terraqueous globe.' Dr. Russell had previously discovered that photographic plates are affected by hydrogen dioxide vapour, which appears to be produced in small amount under the most varying conditions; but Rutherford's exhalations persisted under treatment which would have been fatal to hydrogen dioxide; moreover, these emanations rapidly discharge electrified bodies, a property which hydrogen dioxide does not possess. The existence of such emanations (of which more hereafter) must be borne in mind in forming a judgment of the statements made about these various radiations.

Radio-active substances can communicate transitory radio-activity to all kinds of matter, metals, glass, paper, etc., which then for a short time possess radio-activity equal to ninety times that of uranium. They lose the

property more rapidly, however, when heated or washed. Even distilled water acquires radio-activity, when placed near radium chloride under a glass bell-jar; the water rapidly loses its power in an open vessel after removing it from the proximity of the radium; and even when sealed into a glass tube it loses power after a few days. On the other hand, a solution of a radium salt (*e.g.* radio-active barium bromide) loses activity on exposure to air, but regains it on being kept in a sealed tube.

MM. Curie and Debierne find that this induced radio-activity is greatly increased when the radium compound is placed in a small open vessel under a bell-jar, and sheets of various materials are exposed under the same cover. Even behind leaden screens the activity is induced. If they are in contact with the vessel containing the radium, or with the walls of the enclosed space, only the exposed surfaces are rendered radio-active. The activity of such sheets of material induced by a specimen of barium bromide containing radium, and of which the mean atomic weight of the mixture of metals is 174 instead of 137 (that of barium), is 8000 times that of a piece of uranium of the same dimensions. As long as the sheets are left in the enclosure, the activity persists; if removed, it disappears in a few days. This conveyance of induced radio-activity is equally brought about if the radium compound is placed in one vessel, and the sheets in another, connected with the former by means of a capillary tube; but if communication between the vessels is cut off, the transmission of activity ceases.

It is very remarkable that this transference of radio-activity is confined to radium and actinium; polonium compounds do not appear to possess the property of giving off emanations. It may be that this difference is connected with the fact discovered by Becquerel that while his rays (those of radium and actinium, probably),

like cathode rays, are deviable by a magnet, those of polonium resemble X-rays in being unaffected. Curie, on the other hand, states that both deviable and undeviable rays are emitted from radium as well as from polonium, and that the non-deviable rays are stopped by a piece of thin aluminium foil. None of these rays appear to be polarisable, nor do they show refraction when passed through a prism.

Becquerel also discovered that air, left in contact with some radio-active substances, discharges electrified bodies; indeed, it is impossible to charge an insulated conductor in a room in which any such preparations have been exposed. This power of inducing air to discharge electrified bodies persists for at least a year, even although the preparation has been kept in the dark all the time; it cannot therefore be supposed that light-energy is in any way transformed into such radiations.

In Curie's experiments on induction it was found that provided the vessel containing radium was kept vacuous, the emanations had no longer the property of inducing radio-activity in sheets of metal, etc., exposed in the same vessel. It appears possible, therefore, to pump off the radio-active matter; and the natural conclusion is that it is a gas. The gaseous matter has been collected, or at least air charged with it, and it displays marked chemical action, as well as high radio-activity. It converts oxygen into ozone, and the glass vessels which contain it, if formed of soda-glass, turn violet, and then black, owing to some change. Becquerel, too, remarks on the destructive action of radium rays on the skin; they discolour rock-salt, change yellow phosphorus to red, and destroy the germinating power of mustard and cress seeds.

On the hypothesis that the radiation of radium is produced by the escape of material particles which bombard the walls of the containing vessel, the velocity

of such particles can be determined by a device which may be illustrated thus: Imagine a bullet fired from a rifle placed horizontally, at some little distance above the ground; the bullet will be attracted to the earth, and will fall to the ground after it has gone a certain distance. The factors which determine the spot at which it will strike the ground (excluding the retarding influence of air) are its speed, and the attraction of the earth. If the attraction is known, the speed can be calculated. This analogy illustrates, although imperfectly, the method of arriving at the speed of these impelled particles. They are deviated by a magnetic field, and have a trajectory just as a rifle-bullet has; and their speed has been calculated by Becquerel at 160,000 kilometres or 100,000 miles per second. This estimate differs greatly from the one previously mentioned for cathode rays.

In conclusion, it has been suggested that the existence of such radiations and emanations may be attributed to the existence of 'electrons' in the free state. An electron, it may be explained, is an electric charge which attaches itself to an atom of an element, thereby converting it into an ion. The act of solution in water of such a substance as common salt is now currently held to cause the atom of sodium to separate from the chlorine atom, while each acquires an electric charge, the sodium combining with a positive electron, the chlorine with a negative one, thus: $NaCl + \text{water} + \oplus\ominus = Na\oplus + \text{water} + Cl\ominus +$ water; the neutral molecule of electricity, consisting of two oppositely charged electrons being thus dissociated. Now it is conceivable that such a substance as pitchblende or its radio-active constituents may combine with one of the electrons, liberating the other. It has, indeed, been shown by the Curies that radium rays charge negatively the bodies which receive them, while the radium preparation acquires a positive charge.

Whatever be the true explanation of these mysteries, it cannot be denied that they form the beginnings of what may, and almost certainly will, affect the material future of the human race. When we consider the beginnings made by Gilbert, by Franklin, by Volta, and by Faraday, and contrast them with the outcome of these discoveries, the electric telegraph, and the dynamo machine, we cannot avoid the inference that the future has in store even greater developments than these. It is true that investigators like Hertz, Lenard, Becquerel, and the Curies do not make practical application of their discoveries; but there is never any lack of men who discover their practical value, and apply them to ends useful to mankind. All the more reason, therefore, that every encouragement should be given to the investigator, for it is to him that all our advances in physical and material well-being are ultimately due.

WHAT IS AN ELEMENT?

It was for long held that things around us, animals, vegetables, stones, or liquids, partook of the properties of one or more of the elements—Fire, Air, Earth, or Water. The doctrine was a very ancient one; it probably originated in India; it reached our forefathers through the Greeks. Fire was supposed to be 'hot and dry'; air, 'hot and moist'; water, 'cold and moist'; and earth, 'cold and dry.' And substances which partook of such qualities were supposed to contain appropriate amounts of the elements, which conferred on them these properties.

But in the reign of Charles II. of England, about the year 1660, Robert Boyle, an English philosopher and chemist, restored to the word element the meaning which its derivation implies. 'Element,' or *elemens* in Latin, is supposed to be derived from the three letters L M N; and to denote that, as a word is composed of letters, so a compound is composed of elements. Boyle, in his celebrated work *The Sceptical Chymist*, restricted the use of the word element to the *constituent* of a compound; and that is the meaning which is still attached to the term.

It has often been asked: Does a compound *contain* an element? Are the elements actually *in* the compound? If this means, for example, that iron is present as iron in

iron-rust, the answer must be: No. The properties of rust are wholly different from those of iron; no iron particle can be detected in the rust by any tests which are suitable for the recognition of the metal as such. But if it is meant that iron if exposed to damp air changes into rust, and that by suitable treatment metallic iron can be extracted from rust, then the answer must be in the affirmative.

The fact that an element, when it combines with other elements, entirely loses its original properties, led to the not unnatural supposition that it should be possible to change an element into another, or to transmute it. Long before the notion of 'element' was formulated by Boyle, innumerable attempts had been made to convert one metal into another; and, indeed, it would appear on the face of it to be much easier to transmute lead into silver or gold than to convert it into the yellow earthy powder which it becomes when heated in air. For on the old doctrines, the properties of gold—its lustre, its ductility, its melting in the fire—were much more similar to those of lead than the properties of litharge or oxide of lead, produced by heating lead to redness in air. After Boyle's day, however, it gradually came to be seen that certain substances resisted all such attempts to change them into others *without increasing their weight.* For example, all changes in nature not of a temporary and evanescent character which iron can be made to undergo, are accompanied by an increase in the weight of the iron; they are produced by the combination of iron with other elements, and the addition of another element to iron invariably increases the weight, for the weight of the combining element is added to that of the iron, and the result is a compound differing in properties from iron. It was slowly discovered that about seventy substances must be classed as elements —the minimum number of the present day is seventy-

four—and of these ten are gases, two are liquids, eight elements are usually classed as non-metals, since they do not possess the lustre and some of the other properties of metals; and the remainder are metals. These substances are classified as elements solely because no attempts to convert one into another have up till now been successful; not because such change is in the nature of things impossible. But inasmuch as the properties of these elements, and the changes which they undergo on being brought together with other elements or compounds, have been the subject of an enormous number of experiments, and because no hint of transmutation has been found, the conclusion as regards the immutability of elements has been arrived at. Hence the 'transmutation of elements' has generally been regarded as impossible, and as unattainable as perpetual motion, or as the 'quadrature of the circle.'

Speculation, however, has a deep fascination for many minds; and it has been often held that it is not impossible that all elements may consist of a primal substance—'protyle,' as it has been called—in different states of condensation. It will be worth while to spend a few minutes in considering the reasons for this opinion.

About the beginning of last century, John Dalton revived the old Greek hypothesis that all matter, elements included, consists of atoms or minute invisible particles; these, of course, like the matter which is formed of them, possess weight. Although they are so minute that any attempt to determine their individual weight would be out of the question, Dalton conceived the idea that at least their relative weights could be determined, by ascertaining the proportions by weight in which they are present in their compounds. The compound of hydrogen and chlorine, for example, commonly known as muriatic

or hydrochloric acid, consists of one part by weight of hydrogen combined with $35\frac{1}{2}$ parts by weight of chlorine; and as it is believed to contain one atom of each element, it follows that an atom of chlorine is $35\frac{1}{2}$ times as heavy as an atom of hydrogen. On the same principle, the relative weights of the atoms of other elements were determined. And so, taking the weight of the lightest atom, hydrogen, as unity, the atom of nitrogen weighs 14 times as much, of oxygen 16, of iron 56, of lead 207, and so on.

Attempts to classify elements according to their properties soon followed; and at first the divisions were somewhat arbitrary. The non-metals were distinguished from the metals by their lack of lustre, their feeble power of conducting heat, and the fact that their oxides when mixed with water generally formed acid substances, while those of the metals were earthy, insoluble powders. Certain of the metals, which either do not unite with or are difficult to unite with oxygen at a red heat, were called 'noble' metals; others, which are at once attacked by water, such as sodium and potassium, and which give soapy liquids with a harsh taste, were named 'metals of the alkalies,' and so with the rest. In 1863, however, Mr. John Newlands, a London analyst, was successful in arranging the elements in groups, so that each element in a horizontal column showed analogy with others in the same column. He found that by writing the names of the elements in horizontal rows, beginning with the one of lowest atomic weight, each eighth element possessed properties similar to those of the elements which preceded or followed it in the vertical columns. And in general the composition of the compounds of such similar elements was similar. The first two lines of such a table are reproduced here, so as to show what is meant:—

Name . .	Lithium.	Beryllium.	Boron.	Carbon.	Nitrogen.	Oxygen.	Fluorine.
Atomic Weight .	7	9·1	11	12	14	16	19
Name . .	Sodium.	Magnesium.	Aluminium.	Silicon.	Phosphorus.	Sulphur.	Chlorine.
Atomic Weight .	23	24·4	27·1	28·4	31	32	35·5

If one were to proceed further in the same manner, we should find five elements in the first vertical column—namely lithium, sodium, potassium, rubidium, and cæsium. All of these are soft metals, easily cut with a knife, white in colour like silver, rapidly tarnishing in air, attacked violently by water so that they either catch fire or run about on the surface of the water and rapidly disappear. Their compounds with chlorine each consist of one atom of each element: for example, using Na (natrium) as the symbol for one atom of sodium, and Cl for one atom of chlorine, the composition of the compound of chlorine with sodium (common salt) is expressed by the formula NaCl, implying that the compound is formed of one atom of each element. So with the others: the chloride of lithium is LiCl, of potassium KCl, of rubidium RbCl, and of cæsium CsCl. They all resemble common salt; the taste is similar in all cases, the salts dissolve in water, they are all white in colour, they all crystallise in cubes, and possess many other properties in common. The oxides, too, are all powders, which dissolve in water and give liquids with a soapy feel and a burning taste. For these and other similar reasons, all these elements are believed to belong to the one class.

Let us take an example, too, from the other end of the table. Fluorine, the first of the column, is a pale yellow gas, with a suffocating odour. It combines instantly with hydrogen, yielding a colourless gas, soluble in water, and giving an acid liquid, which corrodes many metals. Chlorine, the second member, is a greenish yellow gas, very similar in properties to fluorine. The third member,

bromine, is a dark red liquid, but at a somewhat lowe temperature than that of boiling water it changes into red gas, with a smell similar to that of chlorine; iodin though a black solid at the ordinary temperature, be comes, when heated, a violet gas. Like fluorine, they a form compounds with hydrogen, of the formulæ HF, HC HBr, and HI; these are colourless gases, soluble in water

Enough has been said to show that Newlands' metho of classifying the elements brings together in vertica columns those that have similar properties. This metho was developed by a German chemist named Lotha Meyer, and by a Russian named Mendeléeff, and it is nov universally acknowledged to be the only rational way o classifying the elements.

If we consider one of the horizontal rows, we shall als discover a peculiarity. The number of atoms of th elements which combine with an atom of oxygen graduall alters; and if they form compounds with hydrogen, th same kind of regularity can be observed. For instance the elements of the first horizontal row given above forn the following compounds with oxygen and hydrogen:

Name . .	Lithium.	Beryllium.	Boron.	Carbon.	Nitrogen.	Oxygen.	Fluorin
Formula of Oxide . .	Li_2O	BeO	B_2O_3	CO_2	N_2O_5	—	—
Formula of Hydride .	LiH	unknown	BH_3	CH_4	NH_3	OH_2	FH

The elements of the subsequent rows show simila regularity.

Up till recently, no elements were known which refuse to combine with other elements. In 1894, however, Lor Rayleigh and Sir William Ramsay discovered tha ordinary air contained such a gas, and they named i 'argon,' a Greek word which signifies inactive or lazy This gas had been overlooked because of its resemblanc to another constituent of the atmosphere, present in nearly one hundred times greater amount—nitrogen

Argon cannot be made to combine, and hence it is left behind when the nitrogen and oxygen have been removed from the atmosphere.

Shortly after the discovery of argon, Ramsay found that certain minerals when heated give off a gas similar to argon, inasmuch as it forms no compounds, but with a much lower atomic weight; for while argon possesses the atomic weight forty, the atomic weight of helium (the name given to this new gas) is only four. Now these elements evidently belong to one series, for they are both colourless gases, incapable of combining with other elements. And it appeared almost certain that other gases, similar in properties to these two, should be capable of existence. And Ramsay, in conjunction with Travers, spent several years in a hunt for the missing elements. They heated upwards of a hundred minerals, to see whether they evolved gas, and, if so, whether the gas obtained was new; but although they discovered that many minerals give off helium when heated, no new gas was found. Mineral waters were boiled, so as to expel dissolved gases; again only argon and helium were obtained. Even meteorites, or 'falling stars,' were heated; only one was found to give off gas incapable of combination, and that gas consisted of a mixture of the two which were already known.

As a last attempt, Ramsay and Travers prepared a large quantity of argon, by removing the oxygen and the nitrogen from air, and then forced the gas into a bulb, dipping in a vessel immersed in a tube full of liquid air, which is so cold that the argon changed to liquid. It forms a colourless, mobile liquid, just like water. When the liquid air is removed, the argon begins to boil.

It was hoped that the distillation of crude liquid argon might separate from it other gases boiling at a lower or a higher temperature; that if it contained any other liquids of lower boiling-point, these would *distil* over first, and

could be collected separately; while any 'heavier' gases would be the last to distil over. The hope was not disappointed—at all events as regards the first expectation; for the first part of the gas which evaporated was considerably lighter than argon and had a much lower boiling-point. After a few redistillations, however, it was found that liquid air was not sufficiently cold to condense this light gas to liquid. But Dr. Travers was equal to the emergency. He constructed an apparatus by help of which hydrogen gas was condensed to liquid; and the boiling-point of liquid hydrogen is much lower than that of liquid air; it is $-252{\cdot}5°$ C. On cooling the mixture of gases which had been separated from the argon, a portion only condensed, while about one-third still remained as a gas; the gaseous portion was helium, and the liquid (or solid) portion evaporated into a gas which was named 'neon,' the Greek word for 'new.'

It was also found that two other gases could be separated from air by allowing a large quantity of liquid air to boil away. These gases have a much higher boiling-point than oxygen, nitrogen, or argon, and therefore they remain mixed with the last drops of liquid after most of the air has evaporated. They were separated from each other by 'fractionation'; one was named 'krypton,' the Greek for 'hidden': and the other 'xenon,' or 'strange.'

Five new gases were thus obtained; they are given with their atomic weight in the following line:

Helium, 4; Neon, 20; Argon, 40; Krypton, 81·6; Xenon, 128.

Their position among other elements is well seen from the following extract from the whole table of the elements:

Hydrogen, 1	Helium, 4	Lithium, 7	Berylium, 9·1, etc.
Fluorine, 10	Neon, 20	Sodium, 23	Magnesium, 24·3, etc.
Chlorine, 35·5	Argon, 40	Potassium, 39·1	Calcium, 40, etc.
Bromine, 80	Krypton, 81	Rubidium, 85·4	Strontium, 87·6, etc.
Iodine, 127	Xenon, 128	Cæsium, 133	Barium, 137·4, etc.

It will be noticed that their atomic weights lie between those of the elements in the vertical rows; and that they separate the active elements of the fluorine group from the equally active elements of the sodium group.

The discovery of these elements, however, has added little to our knowledge as regards the nature of elements in general, except in so far as to show that elements which form no compounds can exist. It might be supposed that the same agencies which are successful in splitting up compounds into the elements of which they consist might decompose elements into some still simpler substances; of course the elements thus decomposed could no longer be called elements. And it appeared not impossible that in a series of elements closely resembling each other, like those of the sodium column, or the chlorine column, it might be impossible to decompose those of higher atomic weight into those of lower atomic weight, and perhaps something else. Such agencies are: a high temperature or an electric current. Water, for instance, can be decomposed into hydrogen and oxygen either by heating steam to whiteness or by passing an electric current through water. But it is needless to say that the elements have been repeatedly exposed to the highest temperature and to the strongest electric currents and yet have remained elements. There are, indeed, reasons for supposing that at the enormously high temperatures of the sun and of the fixed stars some of our elements are decomposed; but it has hitherto been impossible to reproduce such extreme conditions on the earth.

The element carbon is characterised by the enormous number of compounds which it forms, chiefly with hydrogen and oxygen, although many other elements can be induced to combine with it. And one instructive fact is to be noticed as regards such compounds: the greater the number of atoms they contain the more easily they are

decomposed by heat. Indeed, some compounds are so unstable that they decompose at the ordinary temperature, not into their elements, it is true, but into other compounds of carbon, hydrogen and oxygen. Such compounds are stable only at a low temperature, and the higher the temperature the more readily they decompose. Judging by analogy, we should expect elements of high atomic weight to show tendency to decomposition, granting, of course, that any element at all is capable of decomposing. Now among the three elements of highest atomic weight known is radium, an element belonging to the barium column, of which the atomic weight is 226. This remarkable substance exists in a mineral named pitchblende, an oxide of uranium; its discovery by Madame Curie, of Paris, is one of the most remarkable of recent events in chemical history.

The second element of high atomic weight is thorium (232·5). It was noticed by Dr. Schmidt, and independently by Professor Rutherford, of Montreal, that if air was passed over a salt of thorium, or bubbled through its solution, it carried with it an 'emanation' which possessed for a short time the power of discharging an electroscope. Radium salts also give off such an emanation, or gas, which, however, retains its properties for more days than the thorium gas does for minutes. Uranium, the chief constituent of pitchblende, too, has also the power of discharging an electroscope, but it gives off no emanation. Its atomic weight is 239·5: it is the highest known.

The gases evolved from compounds of thorium and radium can be condensed to solid or liquid by passing them through a tube cooled with liquid air. But they are present in such excessively minute quantity that they have never been seen, even as a minute drop. They are as inert as argon, and they are members of that group of

elements; and the radium gas shines in the dark, so that a tube containing it gives off a whitish phosphorescent light like that given off by stale fish, or like the luminosity of the sea on calm summer evenings, or like the head of a lucifer match if it is gently rubbed in the dark. If the gas from radium is mixed with air, it is possible to see it passing through a tube in the dark, and to recognise it by its faint shining when it is transferred from one glass tube to another.

It is very easy to remove oxygen from a mixture of gases; if a piece of the element phosphorus be heated in oxygen, a solid compound of the two is formed, and all oxygen can then be got rid of; or oxygen may be absorbed by passing the mixed gases over red-hot copper. Hence it is convenient to allow the emanation from radium salts to mix with oxygen rather than with air; for nitrogen, the other constituent of air, is more difficult to remove. And it is then possible to collect the radium emanation, mixed with oxygen, in a glass tube, and then to absorb the oxygen, leaving only the emanation present.

Now, as has been said, the emanation gradually loses its power of discharging an electroscope. After four days it requires twice as much emanation to produce the same discharging effect as would be required if the emanation were freshly prepared from radium salts. And the question suggested itself to Mr. Soddy and Sir William Ramsay: What becomes of the emanation? Does it merely lose its luminosity and discharging power, or is it changed into something else?

Chemists have long had at their disposal a means of recognising almost inconceivably minute quantities of matter. All substances, when made into a gas by intense heat, give out light; and that light, if passed through a prism, is seen not often to be all of one kind. For example,

the light given out by sodium gas at a red heat is yellow; and if passed through a slit, and then through a prism, two yellow lines are seen—the spectrum of sodium. Similarly, potassium salts, in a spirit-lamp flame, gives out a violet light; and the prism shows us that the light consists of two kinds—one red and one violet. And so for other elements. If the spectra of gases have to be examined, they can be made to glow by passing an electric discharge through a very narrow tube containing a minute trace of the gas. Helium, for example, if examined in this way, gives out light consisting of many colours: red, yellow—the most intense—green, green-blue, blue and violet. Hence it is easy to recognise the presence of helium in such a capillary tube, by passing an electric discharge through it, for the exact position of the lines in its spectrum is easily recognised.

Now Ramsay and Soddy found that the emanation from radium salts, though it gave out a special light of its own when made luminous by an electric discharge, showed none of the lines characteristic of helium. But after standing for three days the yellow line of helium began to be visible, and that is the one most easily seen. As time went on, and as the emanation lost its self-luminosity, the other lines denoting the presence of helium became distinctly visible. The conclusion was forced upon them, therefore, that, as the emanation disappears, helium is formed, or, in other words, the emanation is changing slowly into helium.

Professor J. J. Thomson, of Cambridge, has of recent years been investigating the motion of particles which are shot off from the negative pole when an electric discharge is passed through gases; and he has succeeded in showing that some of the particles move with enormous rapidity, and that they possess a weight which cannot be much more than one seven-hundredth of that of a hydrogen

atom. It is almost certain that radium salts continually emit such rapidly moving particles, and it is known that while doing so the temperature of the radium salt is some degrees higher than that of the surrounding atmosphere; radium, therefore, is continually giving off heat. We are wholly unacquainted with any similar change; these properties are new. But we do know of compound substances which decompose with slight provocation, give off a great amount of heat in doing so, and at the same time are wholly converted into a large quantity of gases; perhaps the most familiar example is gun-cotton, of which most of the high explosives used for blasting and in the manufacture of modern gunpowder are made. The differences between the two phenomena, moreover, are sufficiently pronounced: gun-cotton decomposes almost instantaneously, with explosive violence; radium salts slowly; gun-cotton requires to be started by the explosion of a percussion-cap; radium salts decompose spontaneously, and the rate of decomposition, so far as is known, appears to be independent of temperature; the amount of heat evolved when gun-cotton explodes, though great in itself, is small in comparison with that evolved during the decomposition of an equal weight of radium salt; and it is not known that any electrical phenomena accompany the decomposition of gun-cotton. Still, it appears reasonable to suspect that the two kinds of change may, after all, be similar, and that the heavy atom of radium is decomposing into the lighter helium atom. It is pretty certain that helium is not the only substance produced when the emanation from radium decomposes; and it is not known whether radium, when it gives off its emanation, produces at the same time any other decomposition product. Much has yet to be discovered. Yet it must be acknowledged that a distinct advance has been made, and that at least one so-called element can no longer be regarded as ulti-

mate matter, but is itself undergoing change into a simpler form of matter.

The young student, when he learns what is known, is too apt to think that little is left to be discovered; yet all our progress since the time of Sir Isaac Newton has not falsified the saying of that great man—that we are but children picking up here and there a pebble from the shore of knowledge, while a whole unknown ocean stretches before our eyes. Nothing can be more certain than this: that we are just beginning to learn something of the wonders of the world on which we live and move and have our being.

ON THE PERIODIC ARRANGEMENT OF THE ELEMENTS

At the end of the eighteenth century, after the investigations of Black, Scheele, Priestley, Cavendish, and Lavoisier began to crystallise the previous arbitrary collections of chemical facts into more or less of a system, it became evident that the distinguishing feature of a 'compound,' as contrasted with a 'mixture,' was the invariability of its composition. Early in the nineteenth century, Dalton formulated his celebrated hypothesis, by means of which a concrete view was gained regarding the cause of this constancy and invariability of composition. Every one knows that this 'explanation' consisted in the supposition that the combination of two substances, one with another, in definite proportions, involves the union either of one atom of the one with one atom of the other, or of certain small but simple numbers of atoms of the two substances. The atom was regarded, not necessarily as indivisible, but as not having been divided into any smaller particles. The advance made by Dalton consisted chiefly in ascribing to each atom a definite weight; but as he had no data for determining the absolute weight of any one atom, he was obliged to content himself with relative weights, and chose the smallest known to him, that of hydrogen, as an arbitrary unit. This choice has proved to be a just one, for as yet no element has been discovered possessing a lower atomic weight than hydrogen, although it is by no means impossible that such an element may exist.

After the convenience of Dalton's hypothesis had been acknowledged, the labour of chemists was for many years devoted to the determination of the relative values of the 'atomic weights' of the elements; or, expressed in a manner independent of hypothesis, of their combining proportions. The name of the Swedish chemist, Berzelius, is prominent in this connection. By the analysis of an almost incredibly large number of compounds, he established on a firm basis the constancy of composition of compounds, and the law of multiple proportions. Towards the 'forties, therefore, a set of numbers had been collected, which invited an attempt to place them in order, with the view of seeing whether some still more profound law could not be discovered connecting the combining numbers attached to them. Döbereiner, as early as 1817, and again in 1829, pointed out that certain elements had atomic weights which were nearly the mean of those of others which were closely related to them; thus, the mean of the atomic weights of calcium and barium gives a close approximation to the atomic weight of strontium; that of sodium lies near the mean of those of lithium and potassium; and sulphur and tellurium similarly indicate selenium as a middle element. In 1843 Gmelin, who published a *Handbook of Chemistry*, which is still a classic, attempted a classification based, not upon numerical relations, but on similarity of properties. For instance, we find the groups—F, Cl, Br, I; S, Se, Te; P, As, Sb; C, B, Si; Li, Na, K; Mg, Ca, Sr, Ba; and so on. In 1851 Dumas gave a lecture before the British Association, in which he showed that not merely is the atomic weight of bromine the mean of those of chlorine and iodine, but that its physical properties, such as its colour, its density in the gaseous and in the liquid state, etc., are also half-way between those of the allied elements. In 1852 Faraday criticised Dumas' attempts as 'speculations which have

scarcely yet assumed the consistence of a theory, and which are only at the present time to be ranked among the poetic day-dreams of a philosopher'; and he proceeded: 'We seem here to have the dawning of a new light indicative of the mutual convertibility of certain groups of elements, although under conditions which are as yet hidden from our scrutiny.'

Passing over attempts by Gladstone, Cooke, Odling, and Strecker, we come to the years 1863 and 1864, when John Newlands, in a series of letters to the *Chemical News*, announced what he termed the 'Law of Octaves.' His actual words were: 'If the elements are arranged in the order of their equivalents, with a few slight transpositions, it will be observed that elements belonging to the same group usually appear on the same horizontal line. It will also be seen that the numbers of analogous elements generally differ, either by 7 or by some multiple of 7; in other words, members of the same group stand to each other in the same relation as the extremities of one or more octaves in music. Thus in the nitrogen group, between nitrogen and phosphorus there are 7 elements; between phosphorus and arsenic, 14; between arsenic and antimony, 14; and lastly, between antimony and bismuth, 14 also. This peculiar relationship I propose provisionally to term the "Law of Octaves."'

In 1869 and 1870, Lothar Meyer and Dmitri Mendeléeff, independently of Newlands, and also of each other, published papers in which they maintained that the properties of the elements are periodic functions of their atomic weights. This discovery goes by the name of the 'Periodic Law,' or better, the 'Periodic System.' The arrangement of Meyer (p. 164), which differs but little from that of Mendeléeff, is the one generally adopted.

If this diagram is rolled round a cylinder, it will form a continuous spiral, beginning with lithium and ending with

I.	II.	III.	IV.	V.	VI.	VII.	VIII.		
								He *4*	
Li 7·03	Be 9·1	B 11·0	C 12·0	N. 14·04	O 16·00	F 19		*Ne* *20*	
Na 23·05	Mg 24·36	Al 27·1	Si 28·4	P 31·0	S 32·06	Cl 35·45		*A* *39·9*	
K 39·14	Ca 40·0	Sc 44	Ti 48·1	V 51·2	Cr 52·1	Mn 55·0	Fe 56·0	Co 59·0	Ni 58·7
Cu 63·6	Zn 65·4	Ga 70	Ge 74	As 75	Se 79·1	Br 79·96		*Kr* *81·5*	
Rb 85·4	Sr 87·6	Y 89	Zr 90·6	Nb 94	Mo 96·0	? 99	Ru 101·7	Rh 103·0	Pd 106
Ag 107·95	Cd 112	In 144	Sn 119·0	Sb 120	Te 127·6	I 126·85		*X* *128*	
Cs 133·0	Ba 137·16	La 138	Ce 140	Prd 141	Nd 143·5	? 152			
? 165	? 170	Yb 173	? 176	Ta 183	W 184	? 185	Os 191	Ir 193	Pt 195·2
Au 197·2	Hg 200·3	Tl 204.1	Pb 206·9	Bi 208·5	? 210	? 211		?	
? 222	Ra 226	? 230	Th 232	? 234	U 240	? 242			

uranium; but there are certain gaps unfilled, denoted by the sign ?, which, it is believed, represent the places of still undiscovered elements. Indeed, Meyer's original diagram contained a larger number of these; and Mendeléeff, averaging the properties of the elements surrounding such gaps, prophesied the discovery of scandium, gallium, and germanium, made at a much later date by Cleve, by Lecoq de Boisbaudran, and by Winckler.

There are many other ways of representing these relations; but except perhaps in convenience (and questionably even in that), they present no particular advantage, and convey no new knowledge. Only one point must be emphasised. The elements, as arranged above, divide themselves into two 'periods'—long periods and short periods. Thus the seventh member after lithium, sodium, is in its character very like lithium; and, again, potassium, the seventh after sodium, presents strong analogies with the two elements named; but it is then necessary to pass over fifteen elements before rubidium is reached, which again closely resembles lithium, sodium, and potassium; and cæsium, the seventeenth element after rubidium, forms the first term of another long period. Copper, silver, and gold are also separated by long periods; and so with the elements in the other columns. To distinguish these in the table, the symbols of the elements in the middle of the long periods are printed towards the left, and those at the beginning towards the right, of the figures denoting the atomic weights.

One other point requires mention. Several instances occur in which the elements appear to occupy a reversed position. Thus, nickel, with the atomic weight 58·7, follows cobalt, to which a higher atomic weight is ascribed; tellurium precedes, instead of following iodine; and it will be seen that argon precedes potassium. The differences between the various consecutive atomic weights are

irregular, and vary between fairly wide limits; and it is quite probable that these differences may occasionally be negative.

In 1894 a new constituent of the atmosphere, which was named 'argon,' was discovered by Lord Rayleigh and Ramsay; this was followed in 1895 by the discovery by Ramsay of helium in certain minerals. This gas gives a spectrum in which a brilliant yellow line is conspicuous. So long ago as 1868 this line had been observed in the solar spectrum by Jansen; it was attributed by Frankland and Lockyer to the presence of a new element in the sun, and they named the then unknown element 'helium.' These discoveries were followed by that of three other gaseous elements in atmospheric air, by Ramsay and Travers in 1898; thus five elements were added to the list. All these elements are distinguished by their inertness, for none of them forms compounds with other elements.

The Roman figures at the head of the columns of the periodic table have a certain significance. They show the maximum number of atoms of hydrogen which the elements in each column can combine with or replace, or, as it is termed, their 'valency.' Thus an atom of lithium combines with one atom of hydrogen; it can also replace one atom, as when it forms lithium hydroxide, $LiOH$, in which it has replaced one atom of hydrogen in water, H_2O. So also magnesium can replace two atoms of hydrogen, for it forms the hydroxide $Mg(OH)_2$. Boron combines with three atoms of hydrogen; carbon with four; phosphorus, although it can combine with only three atoms of hydrogen, can replace five; for it forms a chloride PCl_5, in which it has replaced the five atoms of hydrogen in five molecules of hydrogen chloride, $5HCl$. Sulphur forms a hexafluoride, and iodine a pentafluoride, in which they replace six and five atoms of hydrogen

respectively, in 6HF, and in 5HF. Only one of the elements of the eighth group appears to be able to replace eight atoms of hydrogen, namely, osmium; it forms a tetroxide, OsO_4, thus replacing the eight atoms of hydrogen in four molecules of water, $4H_2O$. But the new gaseous elements of the atmosphere form no compounds, and have no valency, as the power of replacing or combining with hydrogen is termed. They thus form a column by themselves; and it was interesting to ascertain whether their atomic weights would form a series like those in the other columns. In this case, the atomic weight could not be determined by the usual process of determining the ratio in which the elements combine with hydrogen; hence a different method was adopted, depending on the known fact that equal numbers of molecules of gases occupy equal volumes under the same conditions of temperature and pressure; and making use of an argument relating to the number of atoms in such molecules. The atomic weights were:—

Helium	Neon	Argon	Krypton	Xenon
4	20	39·9	81·5	128

These numbers, as will be seen on reference to the table, fit in the eighth column; the symbols and atomic weights of these gases are printed in italics. They form the initial members of the first and second short series, and of the first, second, and third long series.

Some doubt exists as to the place to be assigned to hydrogen, the element with lowest atomic weight. Both Mendeléeff and Meyer shirked placing it. It may be that it should be placed at the head of the fluorine column; but there are equally good, or perhaps better, reasons for believing that it is the first member of the lithium column.

Many attempts have been made to devise some math matical relation between these atomic weights. So lon as there was reason to doubt the accuracy of the exper ments by means of which the atomic weights have bee determined, some such relation as the following ha considerable probability in its favour :—Taking th differences between the atomic weights of the element in the first column, lithium, sodium, potassium, rubidiun and cæsium, they are—

$$\begin{aligned} Na - Li &= 23 - 7 = 16; \\ K - Na &= 39 - 23 = 16; \\ Rb - K &= 85 - 39 = 46 = (3 \times 16) \text{ nearly}; \\ Cs - Rb &= 133 - 85 = 48 = (3 \times 16). \end{aligned}$$

The differences are 16, 16, 3×16, and 3×16. No there are compounds of carbon and hydrogen whic possess the formulæ, CH_4, C_2H_6, C_3H_8, C_4H_{10}, C_5H_{12}, C_6H_1 etc.; and as the atomic weight of carbon is 12, and that c hydrogen 1, the sum of the atomic weights, or, as they ar called, the molecular weights, are respectively 16, 30, 4 58, 72, 86, etc., with a common difference of 14. We se therefore, that a set of compounds may so differ in mole cular weight as to present a regular series, with a commo difference. Nothing was more likely, then, than tha sodium should be regarded as a compound of one atom c lithium with one atom of an unknown element of atomi weight 16, or with two atoms of an unknown element o atomic weight 8; while potassium might be looked upo as a compound of an atom of lithium, with four atoms o the element of atomic weight 8; and so on. But, unfor tunately for this simple theory, the differences betwee the atomic weights of the elements are not exactly equa Instead of 16, the real difference between the atomi weights of lithium and sodium is 16·02; between potassiun

and sodium, 16·09; and so on. In other groups the divergences are still more striking.

The cause of this irregularity has, therefore, to be sought. In seeking for a clue, the first question is: Are the atomic weights invariable? A further question is: Is weight invariable? Does a body always possess the same weight under all conditions? For example, would the weight of a body remain the same if it were to be weighed at different temperatures? Or, if electrically charged, would its weight remain unaltered?

It is a very difficult problem to weigh an object at a high temperature. If the balance, as is usual, contains air, convection currents are produced by the ascent of air heated by the warm body, and the body apparently weighs too little. If the whole balance were uniformly heated, the weights would be at the same temperature as the substance weighed; and it is to be presumed that both they and the substance would alter in weight equally, and still remain in counterpoise. And if the balance case be pumped empty of air, as was done by Crookes in determining the atomic weight of thallium, other phenomena intervene, which, however interesting in themselves (they led Crookes to the invention of the radiometer), are very disconcerting; for attractions and repulsions, which completely disturb equilibrium, are produced by the slightest variations of temperature. However, some curious calculations have been made by Hicks in dealing with Baily's experiments on the attraction of leaden balls by masses of lead—experiments which afford data for calculating the density of the earth. At a high temperature the attraction appeared to be less than at a low one; and as the attraction of the earth is the cause of weight, supposing these experiments to be correct, and the deductions legitimate, it would follow that weight is altered by temperature. The subject is well worthy of further experiment.

Again, interesting experiments have been made by Landolt as regards constancy of weight. Having sealed up in an inverted U-tube two substances capable of acting on each other, such as silver nitrate and sodium chloride, each substance in solution occupying one limb of the tube, he weighed the tube with the utmost accuracy; the possible error might be one part in a million. On inverting the tube, the two solutions mixed, and the reaction took place. It was again weighed. For long, Landolt supposed that he had detected small changes in weight, sometimes negative, sometimes positive; but he was able to trace these changes to the porous nature of glass. On employing tubes made of fused quartz, no change of weight could be detected after the reaction was over. Apparently, therefore, no change of weight takes place as the result of a chemical reaction, provided nothing leaves or enters the vessel in which the reaction goes on.

A very ingenious experiment of Joly's deserves mention. It was designed to try whether any change of mass occurs on mixing two reacting bodies, and the disposition of the apparatus was somewhat like that devised by Landolt. But instead of utilising the attraction of the earth in order to estimate whether the mass had changed or not, the inertia of the substances and of their mixture was determined. The vessel containing the substances to be mixed was suspended to the arm of a torsion-balance, the arm of which was at right angles to the direction of motion of the earth, which is known to be at the rate of about 30 miles a second through space. If matter had been created during the chemical change, then the created matter would not partake of the earth's velocity, and a retardation, made manifest by the rotation of the arms of the torsion-balance in one direction, would have been observed; and if, on the other hand, matter had been destroyed, an acceleration would have shown itself. The experiments

were entirely negative; hence it may be concluded, confirmatory of the experiments of Landolt, that no change in mass is produced by a chemical reaction. A variation in weight or in inertia has not been observed.

There is one curious discrepancy which still remains unexplained. The density of nitrogen gas has been very accurately determined by two very competent observers—Lord Rayleigh and Leduc. They both agree in their results to one part in 10,000. Now it is known, for reasons into which we cannot enter here, that the molecules of both nitrogen and oxygen consist each of two atoms; and as it is also certain that equal volumes of gases contain nearly equal numbers of molecules, when measured under similar conditions of temperature and pressure, the relative weights of these gases correspond to the relative weights of the atoms. The word 'nearly' has been used; for a slight correction must be introduced in order to secure exact correspondence. Hence the atomic weight of nitrogen, referred to that of oxygen taken as 16, as is now customary, must be 14·008, since that is the density of nitrogen referred to oxygen as 16, after the necessary correction has been made. But this number does not correspond with the atomic weight of nitrogen obtained by the celebrated chemist Stas, as the result of the analysis of such compounds as potassium nitrate, when he determined the ratio between the quantities of nitrogen and oxygen in the molecule KNO_3. Both he and, quite recently, one of the most skilful of analysts, to whom we owe in recent years many exact determinations of atomic weights, Theodore Richards, agreed in ascribing the number 14·04 to nitrogen as its atomic weight. The difference does not appear very great; but yet it amounts to one part in 370: and the error of experiment is not likely to be greater than one part in 10,000. This discrepancy is one of the most curious of chemical facts, and it would well repay

further investigation. It may be added that the determination by Gray of the density of nitric oxide, a compound containing one atom of nitrogen in combination with one atom of oxygen, entirely corroborates the results of Lord Rayleigh and Leduc. Experiments are now in progress to combine a weighed quantity of nitric oxide with oxygen, so as to cause it to take up one other atom of oxygen, and to find the increase in weight; and also to remove from it the atom of oxygen, and to find the weight of the oxygen removed; we may, therefore, hope for some explanation of the above discrepancy at no distant date.[1]

The writer of this article was so much impressed by the consideration of this discrepancy, that some years ago, in conjunction with Miss Aston, an attempt was made to find whether the fact of a compound having been formed with absorption, instead of, as is commoner, with evolution of heat, had any influence on the proportions of the elements which it contained. For this purpose the salts of a curious acid derivative of nitrogen named hydrazoic acid, HN_3, were analysed; but there is reason to distrust the results, for it is possible that decomposition occurred during the preparation to some small extent, and so may not have led to trustworthy conclusions. But such as they were, they were in favour of the supposition that the atomic weight of nitrogen in such compounds is less than in those formed with evolution of heat, like the nitre analysed by Stas and by Richards.

An entirely new light has been thrown on the numerical relations of the atoms by the remarkable discovery of radium by the Curies, and by the discovery by Rutherford and Soddy, that what are termed the 'rays' from its salts,

[1] Such investigations have since been carried out by Dr. R. Gray and by Professor Philippe Guye, with the result that the true atomic weight of nitrogen has been fixed as 14·01.

ıs well as from those of thorium, are produced by gases resembling in their inertness the gases of the argon group. These gases, moreover, have the extraordinary property that they are transient, although they change in very different intervals of time. Whereas the gas from thorium s half gone in about a minute (that is, has changed to he extent of one-half into some other substance or substances), that from radium requires about four days before t has undergone half the change of which it is capable. A third gas has been obtained from a radio-active element to which the name 'actinium' has been given by its discoverer, Debierne; this gas has an extraordinarily short ife, for the total duration of its existence is only a few seconds. The spectrum of the gas from radium has been mapped by Ramsay and Collie; the amount of gas produced from a known weight of radium bromide has been measured by Ramsay and Soddy; and they, too, proved that one of its products of decomposition is the lightest gas of the argon group, helium. At first, the spectrum of the emanation from radium shows none of the characteristic lines of helium; but in the course of a few days the helium spectrum appears in full brilliancy. Here, evidently, is a case of the transformation of one element into another; no doubt there are other products than helium, but what they are remains for the present unknown. If they were elements like iron, for example, there are at present no known means delicate enough to detect the extremely minute amount which would be produced. These gases from radium, thorium, and actinium are self-luminous, and shine brilliantly in the dark; and they also possess the power of altering air and other gases with which they are mixed, so that they acquire the property of discharging an electrified body; the air is said to be 'ionised.' But a still more remarkable property is their giving off heat during their change into other elements,

the amount of heat being enormous when their extremely small quantity is considered. Thus the radium emanation (the name applied to the gas which is continuously evolved from salts of radium), during its decomposition gives off no less than three million times the heat which would be evolved during the explosion of an equal volume of a mixture of oxygen and hydrogen in the proportion requisite to form water. Now if radium is disappearing, it must be continually in process of formation, else there would be none on the surface of the earth; it would all have disappeared and have been changed into other bodies during the lapse of time since the minerals containing it were formed. As radium is always associated with uranium, it appears not unreasonable to suppose that uranium, too, which is a radio-active element, is slowly changing into radium; and there appears to be definite ground for the surmise that polonium, the first of the radio-active elements, also discovered by Madame Curie, which has a half-life period of about one year, is a product of the decomposition of radium, with which it is always associated. It may be mentioned, too, that all minerals containing uranium contain more or less helium.

It will be noticed, on referring to the periodic table, that all the radio-active elements, that is, all those which are undergoing change of the nature described, have very high atomic weights. That of uranium is 240; that of thorium, 232; and that of radium, 226. Now it is a commonplace of the organic chemist that it is not possible to build up compounds of carbon and hydrogen of unlimited complexity; indeed, it is doubtful if any compound has been prepared containing more than 100 atoms of carbon. Attempts to prepare them lead to failure, owing to their decomposing at the ordinary temperature into compounds containing a smaller number of atoms. And it is probable that more complex hydrocarbons, as such com-

pounds are termed, would, if they could exist, decompose with evolution of heat. Such a decomposition appears to present analogy with the change which an element like radium is undergoing. It is in process of change into other elements of lower atomic weight; and in changing, it evolves heat, in amount enormously greater than that produced by any change of a compound into a mixture of simpler compounds. But the matter is complicated by another phenomenon—that of discharging with almost inconceivable velocity particles which appear, according to J. J. Thomson, to be identical with negative electricity. These 'corpuscles,' as they have been termed, embed themselves in the vessel in which the radio-active body is confined; and, owing to their extreme minuteness, they may even pass through the walls of the containing vessel. Indeed the opposition of their passage has been shown to depend merely on the density of the matter of which the confining walls are composed; gold, which is denser than lead, stops their passage better than lead; for a similar reason lead is better than iron, iron better than glass, and so on. Thomson has calculated that the mass of one such particle is approximately one-thousandth of that of an atom of hydrogen.

This new chemistry is just at its commencement. It dates from 1896, when Becquerel showed that compounds of uranium evolved some sort of radiation which would impress a photographic plate. It is still too early to formulate any definite statement relating to its connection with the irregularity in the numerical sequence of the atomic weights; yet it may be permissible to speculate, aided by the recent discoveries. When two elements combine, heat is generally evolved; now heat is only one form of energy, and the combination of elements may be so carried out as to be accompanied by other kinds of energy—for instance, by the production of an electric

current. Conversely, when a compound is resolved into its elements, it is generally necessary to impart energy to it; and the element may, therefore, be said to 'contain' more energy than its compounds. Now, as Ostwald has pointed out in his 'Faraday' lecture, the progress of discovery has kept pace with the amount of energy with which it was possible at the time to load a compound; and he cited the discovery of the metals of the alkalies, sodium and potassium, by Davy. It was because Davy had at his disposal the powerful battery of the Royal Institution, that he was able to convey enough energy into caustic potash to isolate from it potassium, hydrogen, and oxygen. If we assume that radium, as may be possible, is produced by a spontaneous change in uranium; and if we also assume that radium contains more energy than uranium; then as such a spontaneous change must be accompanied, on the whole, by a loss of energy, there must be formed other bodies from the uranium which contain less energy than it does. Such a substance may be iron, which is generally found in company with uranium. If we could concentrate energy into iron, it might be possible to convert it into uranium.

But there is another side to this question. The nature of the energy required appears to be electric in character. Now it is almost certain that negative electricity is a particular form of matter; and positive electricity is matter deprived of negative electricity—that is, minus this electric matter. The addition of matter in any form would, according to all experience, increase mass; it would also increase weight. It is, therefore, conceivable that an element may consist of a compound of two or more elements of lower atomic weight, plus a certain quantity of negative electricity. This might account for the approximate numerical relations which subsist between the atomic weights of the nearly related elements; and also for the fact

that the relation is not an exact one, but only approximate; for the difference between the actual atomic weight, and that which would follow if one element were a compound of other elements of lower atomic weights, would be caused by the addition of a certain number of electric atoms to the molecule.

It must be confessed, however, that the basis for speculations like these is a slender one; the sole ground is the undoubted fact that radium produces an emanation which spontaneously changes into helium; and also that, in doing so, the emanation parts with a large number of corpuscles carrying negative charges. Nevertheless, enough is known to prove that there is a wide field for experiment, and that the harvest will be a rich one; further, the reapers' task will be one of extraordinary interest.

RADIUM AND ITS PRODUCTS

CHEMISTRY and physics are experimental sciences; and those who are engaged in attempting to enlarge the boundaries of science by experiment are generally unwilling to publish speculations; for they have learned, by long experience, that it is unsafe to anticipate events. It is true they must make certain theories and hypotheses. They must form some kind of mental picture of the relations between the phenomena which they are trying to investigate, else their experiments would be made at random and without connection. Progress is made by trial and failure; the failures are generally a hundred times more numerous than the successes; yet they are usually left unchronicled. The reason is that the investigator feels that even though he has failed in achieving an expected result, some other more fortunate experimenter may succeed, and it would be unwise to discourage his attempts.

In framing his suppositions, the investigator has a choice of five kinds; they have been classified by Dr. Johnstone Stoney. 'A theory is a supposition which we hope to be true, a hypothesis is a supposition which we expect to be useful; fictions belong to the realm of art; if made to intrude elsewhere, they become either make-believes or mistakes.' Now the 'man in the street,' when he thinks of science at all, hopes for a theory; whereas the investigator is generally contented with a hypothesis, and it is only after forming and rejecting numerous hypo-

theses that he ventures to construct a theory. He has a rooted horror of fiction in the wrong place, and he dreads lest his hypothesis should turn out to be misplaced fiction.

I have thought it better to begin by these somewhat abtruse remarks, in order to place what I propose to discuss on a true basis. It is to be understood that any suppositions which I shall make use of are of the nature of hypotheses, devised solely because they may prove useful. Events are not yet ripe for a theory.

It will be remembered that Professor Rutherford and Mr. Soddy announced a 'view' that certain elements which possess the power of discharging an electroscope, and which are therefore called 'radioactive,' are suffering disintegration—that is, they are splitting up into other elements, only one of which has as yet been identified. Three of these elements, namely, radium, thorium, and actinium, early in the process of disintegration give off an 'emanation,' or supposed gas; the proof of the gaseous nature of these emanations is that they can be confined by glass or metal, like gases, and that they can be liquefied or solidified when cooled to a sufficiently low temperature. It is necessary to pay attention to this peculiarity; for these radioactive elements, and two others, uranium and polonium, also give off so-called β-rays, which penetrate glass and metal, and which are believed from the discoveries of Professor J. J. Thomson and others to be identical with negative electricity.

Now Rutherford and Soddy, reasoning on the premises that radium was always found associated with uranium and thorium, and also that the ores of these metals, pitchblende and thorite, had been found to contain the gas helium, made the bold suggestion, 'The speculation naturally arises whether the presence of helium in minerals and its invariable association with thorium and uranium

may not be connected with their radioactivity.' Besides the premises already mentioned, they had evidence of the probable mass of the 'a-particles,' which appeared to be about twice that of an atom of hydrogen. Now helium is the lightest gas next to hydrogen; and its atoms are four times as heavy as atoms of hydrogen. It was, therefore, a striking confirmation of the accuracy of this view when Ramsay and Soddy discovered that helium can actually be obtained from radium.

Before giving an account of that discovery, a short description of the nature and properties of helium may not be out of place. When light passes through a prism, it is refracted or bent; and Newton discovered that white light, such as is emitted from the sun or the stars, after passing through a prism, gives a spectrum consisting of coloured images of the hole in the window-shutter through which the sunlight fell on his prism. Fraunhofer, a Berlin optician, conceived the idea of causing the light to pass through a narrow slit, instead of a round hole; and the spectrum then consisted of a number of images of the narrow slit, instead of the round hole. He was struck by one peculiarity shown by sunlight when thus examined, namely, that the coloured band, rainbowlike, and exhibiting a regular gradation of colour from red at the one end, through orange, yellow, green, and blue, to violet at the other end, was interspersed by very numerous thin black lines. The nature of these lines was discovered by Kirchoff. The light emitted by a white-hot body shows a continuous spectrum; but if such white light be passed through the vapours of a metal, such as sodium, a portion is absorbed. For example, glowing sodium gas shows two yellow lines, very close together; but if this light is passed through the vapour of sodium, these lines are extinguished if the correct amount of vapour be interposed. Now it was found that the position of the two dark lines in the

sun's spectrum, discovered by Fraunhofer, is identical with that of the two yellow lines visible in the spectrum of glowing sodium vapour; and Kirchoff concluded that this coincidence furnished a proof of the presence of sodium in the sun. Fraunhofer had named these lines D_1 and D_2. Similar conclusions were drawn from observations of the coincidence of other black solar lines with those of elements found on the earth; and the presence of iron, lead, copper, and a host of elements in the sun was proved.

In 1868 a total eclipse of the sun took place; an expedition was sent to India, from which a good view was to be obtained. Monsieur Janssen, the distinguished French astronomer, observed a yellow line, not a dark, but a bright one, in the light which reached the earth from the edge or 'limb' of the sun, and which proceeded from its coloured atmosphere or chromosphere. It was for some time suspected that this line, which was almost identical in position with the yellow lines of sodium, D_1 and D_2, and which Janssen named D_3, was due to hydrogen. But ordinary hydrogen had never been found to show such a line; and after Sir Edward Frankland and Sir Norman Lockyer had convinced themselves by numerous experiments that D_3 had nothing to do with hydrogen, they ascribed it to a new element, the existence of which on the sun they regarded as probable; and for convenience, they named this undiscovered element 'helium,' from the Greek word for the sun, ἥλιος.

It was not until the year 1895 that helium was found on the earth. After the discovery of argon in 1894, Ramsay repeated some experiments which had previously been made by Dr. Hillebrand, of the United States Geological Survey. Hillebrand had found that certain minerals, especially those containing the somewhat rare

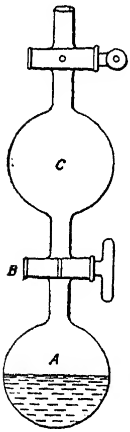
elements uranium and thorium, when heated, or when treated with acids, gave off a gas which he took for nitrogen. But the discovery of argon had taught Ramsay how to deal with such a gas. He examined it in the hope that it might lead to the discovery of a compound of argon; but its spectrum turned out to be identical with that of solar helium, and terrestrial helium was discovered. It proved to be a very light gas, only twice as heavy as hydrogen, the lightest substance known; its spectrum consists chiefly of nine very brilliant lines, of which D_3 is the most brilliant; it has never been condensed to the liquid state, and is the only gas of which that can now be said (for hydrogen has been liquefied within the last few years[1]), and, like argon, it has not been induced to form any chemical compound. That it is an element is shown by the relation of its atomic weight, 4, to that of other elements, as well as by certain of its properties, the most important of which is the ratio between its specific heat at constant volume and constant pressure; but to explain the bearing of this property on the reasoning which proves it to be an element would be foreign to the subject of this article.

This then was the elementary substance that Rutherford and Soddy suspected to be one of the decomposition products of radium. The word 'decomposition,' however, implies the disruption of a compound, and the change which takes place when radium produces helium is of such a striking nature that it is perhaps preferable to use the term 'disintegration.'

Having procured fifty milligrammes (about three-quarters of a grain) of radium bromide, Ramsay and Soddy placed the greyish-brown crystalline powder in a small glass bulb about an inch in diameter. This bulb was connected by means of a capillary tube with another

[1] It has since been liquefied by Kammerlingh Onnes of Leiden.

bulb of about the same size; on each side of the secon bulb there was a stop-cock, as shown in the sketch. T begin with, the bulb *A* was pumped empty of air; i contained the dry bromide of radium. Th stop-cock *B* was then shut. Next, some water was placed in bulb *C*, and it too was pumped free from air, and the stop-cock *D* was closed. *B* was then opened, so that the water in *C* flowed into *A*, and dissolved up the bromide of radium. As it was dissolving, gas bubbles were evolved with effervescence, and that gas collected in the two bulbs, *A* and *B*. The sketch shows the state of matters after the water had been added and the gas evolved. The apparatus was then permanently sealed on to a tube connected with a mercury-pump, so contrived that gas could be collected. The stop-cocks having been opened, the gas passed into the pump, and was received in a small test-tube. From the test-tube it was passed into a reservoir, where it was mixed with pure oxygen, and electric sparks were then passed through it for some hours, a little caustic soda being present. This process has the result of causing all gases except those like argon to combine, and they are therefore removed. It was easy to withdraw oxygen by heating a little phosphorus in the gas; and it was then passed into a small narrow glass tube, which had a platinum wire sealed in at each end—a so-called Plücker's vacuum-tube. On passing an electric discharge from a Ruhmkorff induction-coil through the gas in the tube, the well-known spectrum of helium was seen.



EXTRACTION OF GASES FROM RADIUM BROMIDE

Thus helium was proved to be contained in radium bromide which had stood for some time. The specimen used was said to be about three months old, and the

helium had accumulated. But whence came the helium? That was the next question to be settled.

A solution of radium bromide gives off gas continuously. That gas, on investigation, is found to be a mixture of oxygen and hydrogen, the constituents of the water in which the bromide is dissolved. It contains, however, a small excess of hydrogen, which implies that some oxygen has been absorbed, probably by the radium bromide, although what becomes of that excess has not yet been determined.

When an electric spark is passed through a mixture of oxygen and hydrogen, an explosion takes place; the gases combine, and water is formed. Any excess of hydrogen is, however, unaffected. Now the gases evolved from a solution of radium bromide make glass luminous in the dark, and possess the power of discharging an electroscope, like radium bromide itself. Rutherford and Soddy discovered that when this mixture of gases is led through a tube shaped like a U, cooled to $-185°$ C. by dipping in liquid air, the luminous gas condenses, and the gases which pass on have nearly ceased to be luminous in the dark, and no longer discharge an electroscope. To such condensable gases Rutherford applied the term 'emanation'; this one is known as the 'radium emanation.'

The next question to be answered was: Is the helium evolved from the radium bromide directly, or is it a product of the emanation? It was necessary, therefore, to collect the emanation and to examine its spectrum. This was managed, after many unsuccessful trials, by exploding the mixture of oxygen and hydrogen containing the emanation, allowing the remaining hydrogen to pass into a tube containing a thin spiral of slightly oxidised copper wire kept at a red heat by an electric current: the hydrogen combined with the oxygen of the oxide of

copper, and formed water. The apparatus was so arrange that mercury could be allowed to enter the tube fron below, so as to sweep before it any remaining gas; an the water was removed from the gas by making it pas through a tube filled with a suitable absorbing agen followed up by mercury. The gas finally entered a ver small spectrum-tube, entirely made of capillary tubin like the stem of a thermometer. On passing a discharg from a coil through the spectrum-tube after the emanatio had been thus introduced, a spectrum was seen, consistin of some bright green lines; but it was extremely difficul to prevent the presence of traces of carbon compound and at this stage their spectrum was always seen. Bu the D_3 line of helium was absent. After a couple of day however, a faint yellow hue began to appear, identical i position with D_3; and as time went on, that line becam more distinct, and was followed by the other lines charac teristic of helium, until, after a week, the whole heliun spectrum was visible. It was thus proved that the radiun emanation spontaneously changes into helium. Of cours other substances might have been, and undoubtedly wer formed; but these it was not possible to detect.

The next problem was to measure the amount c emanation, resulting from a given weight of radium, in given time. The method of procedure was similar to tha already described, except in one respect: the spiral c oxidised copper wire was omitted, and the excess c hydrogen, mixed with the emanation, was cooled in a sma bulb by help of liquid air. This condensed the emanatio and the hydrogen, which of course is not liquefied at th temperature of liquid air, was pumped away. On remova of the liquid air, the emanation became gaseous, and i was forced by means of mercury into a minute measurin tube, like the very narrow stem of a thermometer. I was thus possible to measure its volume. It is a well

known law that gases decrease in volume proportionally to increase of pressure; if the pressure is doubled, the volume of the gas is halved, and so on. Now this was found to be the case with the emanation; hence the conclusion that it is a gas, in the ordinary meaning of the word. But it is a very unusual gas; for not only is it luminous in the dark, but it slowly contracts, day by day, until it practically all disappears. It does not lose its luminosity, however; what remains, day by day, is as luminous as ever; but its volume decreased, until after about twenty-five days the gas had contracted to a mere luminous point. What had become of the helium? That was discovered on heating the tube. It is well known that glass, exposed to the radium emanation, turns purple, if it is soda glass; brown, if it is potash glass. This is due to the penetration of the glass by the electrons, which are exceedingly minute particles, moving with enormous velocity. When the emanation changes into helium, the molecules of that gas are also shot off with enormous velocity, although they move much more slowly than the electrons. It is sufficient, however, to cause them to penetrate the glass; but on heating they are evolved, and collect in the tube, and the volume of the helium can be measured. It turned out to be three and a half times that of the emanation. But as the emanation is probably fifty times as heavy as hydrogen, all the emanation is not accounted for by the volume of helium found; it is almost certain that solid products are formed, which are deposited on the glass, and which are radioactive. Up to the present these products have not been investigated chemically.

It was possible, knowing the volume of the emanation, and knowing also the volume which the radium would have occupied had it, too, been gaseous (for a simple rule enables chemists to know the volume which a given

weight of any element would occupy in the state of gas), to calculate how long it would take for the radium to be converted into emanation, supposing that to be its only product. This gives for half of the radium to be decomposed about 1150 years. But there is a good deal of conjecture about the calculation; for many unproved assumptions have to be made.

A further experiment, conducted in a somewhat similar manner, but with the utmost precaution to exclude every trace of foreign gas, made it possible to measure the position of the lines of the spectrum of the emanation. In general it may be said that the spectrum has a similar character to those of argon and helium; it consists of a number of bright lines, chiefly green, appearing distinctly on a black background. It confirms the supposition, made after examination of the chemical properties of the emanation, that it is a gas belonging to the argon group, with a very heavy atomic weight. Some of the lines of the spectrum appear to be identical with lines observed in the spectra of the stars; and it may perhaps be inferred that such heavenly bodies are rich in radium.

In the diagram on page 184, the bulb containing radium bromide there shown was surrounded by a small glass beaker, as a precautionary measure. As a matter of fact, there were three such bulbs and three such beakers, on the principle of not putting all one's eggs in one basket. These beakers had never been in contact with the radium bromide, nor with the emanation; but they had been bombarded for months by β-rays, or electrons, which are so minute, and move so rapidly, that they penetrate thin glass with ease. It was found that these beakers were radioactive; and it is very remarkable that after washing with water, the beakers lost their radioactivity, which was transferred to the water. Evidently, then, some radioactive matter had been produced by the

influence of the β-rays. On investigation, it was proved that more than one substance had been produced. For on bubbling air through the water, a radioactive gas passed away along with the air; it had the power of discharging an electroscope, but its life lasted only a few seconds. It was only while the current of air was passing through the electroscope that the gold-leaves fell together; on ceasing the current, the leaves remained practically stationary. Now had radium emanation been introduced into the electroscope, its effect would have lasted twenty-eight days; had the emanation from thorium been introduced, it would have taken about a minute before it ceased to cause the gold-leaves to fall in. There is an emanation, however, that from actinium, which is very short-lived, and it looks probable that one of the substances produced from the β-rays is actinium. But it is not the only one. For the water with which the glass was washed gives a radioactive residue after evaporation to dryness; and it contains a substance which forms an insoluble chloride, sulphide, and sulphate, though the hydroxide is soluble in ammonia. Either, then, the β-rays have so altered the constituents of the glass that new radioactive elements are formed; or perhaps it is the air which surrounds the glass which has yielded these new elements; or it may be, though this appears less probable, that the β-rays themselves, which are identical with electrons, or 'atoms' of negative electricity, have condensed to form matter.

Such are some of the results which have been obtained in a chemical examination of the products of change of radium. The work is merely begun, but it leads to a hypothesis as regards the constitution of radium and similar elements, which was first put forward by Rutherford and Soddy. It is that atoms of elements of high atomic weight, such as radium, uranium, thorium, and the suspected elements polonium and actinium, are unstable;

that they undergo spontaneous change into other forms of matter, themselves radioactive, and themselves unstable; and that finally elements are produced which, on account of their non-radioactivity, are, as a rule, impossible to recognise, for their minute amount precludes the application of any ordinary test with success. The recognition of helium, however, which is comparatively easy of detection, lends great support to this hypothesis.

The natural question which suggests itself is: Are other elements undergoing similar change? Can it be that their rate of change is so slow that it cannot be detected? Professor J. J. Thomson has attempted to answer this question, and he has found that many ordinary elements are faintly radioactive; but the answer is still incomplete, for, first, radium is so enormously radioactive that the merest trace of one of its salts in the salt of another element would produce such radioactivity; and, second, it is not proved that radioactivity is an invariable accompaniment of such change; or, again, it may be evolved so slowly as to escape detection. A lump of coal, for example, is slowly being oxidised by the oxygen of the air; oxidation is attended by a rise of temperature, but the most delicate thermometer would detect no difference between the temperature of a lump of coal and that of the surrounding air, for the rate of oxidation is so slow.

Another question which arises is: Seeing that an element like radium is changing into other substances, and that its life is a comparatively short one, it must be in course of formation, else its amount would be exhausted in several thousand years. An attempt has been made by Soddy to see if uranium salts, carefully purified from radium, have reproduced radium after an interval of a year; but his result was a negative one. Possibly some other form of matter besides uranium contributes to the synthesis of

radium, and further experiments in this direction will be eagerly welcomed.[1]

Lastly, the experiments of Ramsay and Cook, of which an account has been given, on the action of the β-rays appear to foreshadow results of importance. For while radium, during its spontaneous change, parts with a relatively enormous amount of energy, largely in the form of heat, it is a legitimate inference that if the atoms of ordinary elements could be made to absorb energy, they would undergo change of a constructive and not of a disruptive, nature. If, as looks probable, the action of β-rays, themselves the conveyers of enormous energy, on such matter as glass, is to build up atoms which are radioactive, and consequently of high atomic weight; and if it be found that the particular matter produced depends on the element on which the β-rays fall, and to which they impart their energy:—if these hypotheses are just, then the transmutation of elements no longer appears an idle dream. The philosopher's stone will have been discovered, and it is not beyond the bounds of possibility that it may lead to that other goal of the philosophers of the dark ages—the *elixir vitæ.* For the action of living cells is also dependent on the nature and direction of the energy which they contain; and who can say that it will be impossible to control their action, when the means of imparting and controlling energy shall have been investigated?

[1] There appears to be an intermediate product to which the name 'ionium' has been given by Boltwood, its discoverer.

WHAT IS ELECTRICITY?

An old friend of mine, by profession a banker, who spent a large portion of his life of eighty-nine years in studying geology and astronomy, once put to me the question: 'Whence comes the motive power of electricity? I can understand the motive power of steam, but not of electricity.'

This led me to think on the subject; and although there is not much new in my reply, it contains, nevertheless, one novel point, which contributes, I think, to clearness of thought.

The answer refers only to electricity generated by a battery; not to a current made by means of a dynamo machine. The answer to the question, What generates a current in a dynamo? must be left till a later opportunity.

The simplest form of a battery consists of a vessel containing dilute hydrochloric acid, into which dip a copper and zinc plate, connected by a wire. A current flows through the wire; its presence can be demonstrated by a galvanometer, or by dipping the wire from the copper plate and the wire from the zinc plate into a solution of iodide of potassium; a brown stain begins to appear at the end of the wire connected with the zinc plate; it is caused by the iodine being set free, which dissolves in the liquid with a brown colour.

If it is desired to make the test more striking a little starch may be added to the solution of iodide of potassium.

The colour will then be blue, for iodine and starch give a blue colour. Now why does the current pass?

To explain this, let us consider what happens to a lump of sugar lying at the bottom of a cup of water. After a few minutes the sugar will melt, or, more correctly, dissolve in the water. But the water at the top will not be sweet for a long time; the sugar takes a good many minutes before it spreads up into the water. Why? It is believed that sugar consists of minute invisible particles called molecules; and they are in motion.

Although we cannot see molecules move, we may nevertheless make an experiment which will prove to us that particles of matter, easily visible under a fairly powerful microscope, are always in rapid motion.

An ordinary water-colour paint, rubbed with water, gives particles of a convenient size; gamboge is perhaps the best colour to take. These particles are always 'jigging' to and fro; their motion is not regular, but spasmodic; and they spread, in virtue of that motion; so that they move from one part of the water to another.

So it is with the sugar molecules; that they do spread is proved by the water becoming sweet, even at the surface. In fact the sugar particles try to move from where they are to where they are not. If one felt inclined to moralise on the subject, one might ask, Is not that what we all try to do? Is not an attempt at motion what makes for progress of all kinds in the world?

If such motion could be hindered, say by a screen which would block the passage of the sugar molecules, while allowing the water molecules to pass, the sugar molecules would bombard the screen, giving it innumerable blows, and these blows would make themselves evident as a kind of pressure on the screen.

This pressure has been measured; a partition has been

found which allows the water to pass, while blocking the way for sugar. It is as if gravel of two sizes were being shaken on a sieve; the stones which pass through the meshes do not press on the sieve, while those which are stopped by the sieve may be recognised by their pressure.

Substances other than sugar, too, can be stopped by the same screen; for example, tartaric acid can. And it has been found that the pressure produced by equal numbers of molecules or particles of sugar and of tartaric acid, contained in equal volumes of water, is equal.

Common salt is a compound of a metal named sodium and a yellow-green gas called chlorine. Each molecule or particle of salt must therefore contain these two elements; that is, each particle must be made up of at least two smaller particles, and these smaller particles are called 'atoms.' If a spoonful of salt be placed at the bottom of a glass of water, like the sugar, its particles will wander through the water, so that, after some time, the water will become salt all through.

Just as with sugar, it is possible to find a membrane which will allow water to pass through it, while it stops the passage of salt; and it is possible to measure the pressure of molecules of salt on the membrane.

Now here a very curious thing has been found; molecules of salt give twice as great a pressure as an equal number of particles of sugar, spread through the same volume of water; it looks as if there were twice as many particles of salt present. And it is supposed that there really are twice as many. To account for this, it is believed that each molecule of salt splits up into two atoms, one of sodium and one of chlorine, and that each atom plays the part of a molecule, in so far as it is able to raise pressure. Owing to the habit which such minute particles as the atoms of sodium and chlorine have of

moving about in a watery solution, they are named 'ions,' a Greek word, which means 'wanderers.'

But an ion is not merely a wandering particle; the moving particles of sugar are not called ions. The ions contained in a solution of salt have another peculiarity; one has gained, and the other has lost, what we may term an atom of electricity. Now what is electricity?

It used to be believed, formerly, that there were two kinds of electricity, one called positive and the other negative. At that time it would not have been possible to answer the question. But recent researches make it probable that what used to be called negative electricity is really a substance. Indeed the relative weight of its particles has been measured; each is about one seven-hundredth of the mass of an atom of hydrogen: and the mass of an atom of hydrogen is the smallest of all masses of what we have been used to call matter.

Atoms of electricity are named 'electrons'; they appear to be all of one kind. The metal sodium, and indeed all other metals, may be regarded as compound of electrons with a stuff which may be named 'sodion' for sodium, 'cuprion' for copper, 'ferrion' for iron, and so on. When sodium loses an electron it becomes 'sodion'; when iron loses three electrons it becomes 'ferrion,' and similarly with the rest.

How can sodium be made to lose its electron? This happens when it enters into combination. When sodium is heated in air, which contains oxygen gas, it burns, and is said to unite or combine with oxygen; burning appears to be accompanied by a transference of an electron from the sodium to the oxygen. Common salt may be made by heating sodium in chlorine gas; it takes fire, burns and is changed into white ordinary salt. It has lost an electron; chlorine has gained one.

When dissolved in water, the sodium exists in the water

as sodion; that is, sodium less an electron. The chlorine is in the water, not as chlorine; by gaining an electron, it has been converted into chlorion. We see, therefore, that those elements which we call metals become ions by losing electrons; while those which we call non-metals become ions by gaining electrons.

Let us now consider the simple battery or cell, consisting of a plate of copper and a plate of zinc, dipping in a jar half full of dilute hydrochloric acid. This hydrochloric acid consists of a number of ions of hydrogen; and ions of hydrogen differ from ordinary hydrogen gas in the same way as ions of sodium differ from metallic sodium, namely, by each atom having parted with an electron. The electron which each atom has lost has attached itself to an atom of chlorine, and the chlorine atom is thereby converted into an ion.

The plate of zinc cannot dissolve in the water, until its atoms have been converted into ions. They would then each have to part with two electrons. But the attraction of an atom of zinc for these two electrons is so great that the zinc does not dissolve, unless, indeed, the electrons can be conveyed elsewhere.

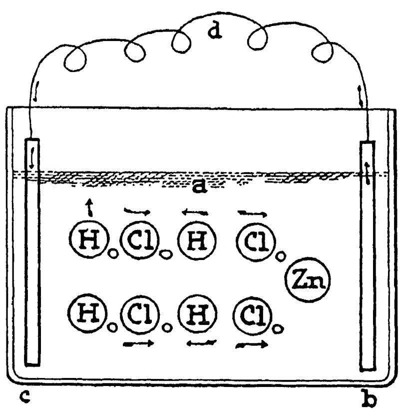
Now electrons have the power of travelling through metal; this point will be considered later; it must be accepted for the present. When an atom of zinc gives up its two electrons to the zinc plate, the atom of zinc which lies nearest to that which has parted with these two electrons will be overloaded; it already is in combination with its own two, and cannot unite with two additional ones; or, if it does, it must pass on its own electrons to the neighbouring atom.

These two electrons, therefore, displace others, or, it may be, are themselves transmitted through the zinc, until they reach the copper wire. Copper, in the metallic state, is also a compound of copper ions with two elec-

trons; and the copper, like the zinc, is overloaded by the electrons from the zinc. Hence it transmits them to the copper plate, and they find their way to the surface of the plate.

There they find hydrogen ions, which are ready to combine each with one electron in order to form hydrogen atoms; and having combined, the atoms of hydrogen unite in couples, bubbles of hydrogen are formed, and float up to the surface and burst. In short, the zinc passes on its electrons through the copper wire to the copper plate, when they are transmitted to the ions of hydrogen in solution, and these first become atoms and then molecules.

These conceptions, which are rather intricate, may be rendered clearer by means of a diagram. *a* is the solution of hydrochloric acid in water; *b* is the zinc plate; *c* is the copper plate, and *d* the connecting wire. *H H*, on the left of the diagram, are two atoms of hydrogen, each



EXPLANATION OF A GALVANIC CELL

of which has gained an electron; they will unite together to a molecule, and escape in a bubble up through the

liquid. The electrons which they have gained have followed the arrows from the zinc plate, along the copper wire and down the copper plate.

A zinc atom minus its two electrons has left the zinc plate; it is now a zinc ion. These two electrons have displaced other electrons from their combination with zinc and copper; and it is these electrons, or their substitutes, which have attached themselves to the hydrogen ions. There are hydrogen and chlorine ions in the liquid. The hydrogen ions move toward the copper plate, and the chlorine ions toward the zinc plate, but less rapidly.

Some of these will touch the zinc plate; and if they could pass round the circuit, through the wire, there would be no electric pressure; but it is because the plates and the connecting wire are impervious to matter, while they are pervious to electrons, that electric pressure—or to give it the usual name, electromotive force or potential—is developed. In fact, the metals and the wire are semi-permeable membranes; they allow electrons to pass, while they block the passage of matter.

Perhaps the idea may be somewhat more easily grasped if it is put in another form. Electrons do not pass through water; probably because the treble combination of electrons, hydrogen, and oxygen is too firm to allow of the transference of electrons from one molecule to another. But when a salt is dissolved in the water electrons can pass, for they easily transfer themselves from one place to another, carrying along with them atoms such as chlorine. Their progress is much impeded thereby; but, as explained before, they are easily transmitted through metals, and thus, again, electric pressure is developed.

The analogy with 'osmotic pressure,' as the pressure of the sugar molecules dissolved in water against a semi-permeable membrane is called, is obvious; just as the water in which the sugar is dissolved can pass in and out

through the semi-permeable screen or partition, so the electrons can pass backwards and forwards through the metallic plates and wire; and just as the sugar molecules are unable to traverse the membrane, so the matter with which the electrons are in combination is unable to pass through the metal. The metal is thus a semi-permeable membrane, and electric pressure is developed in consequence, in the same way as osmotic pressure is developed by the sugar in solution.

If a weak solution of common salt be boiled down, after sufficient water has been evaporated away, crystals of salt separate out and deposit. Now the weak solution contains the constituents of the salt almost entirely in the state of ions; that is, the sodion is without an electron, which, if added, would convert it into the metal sodium; and the chlorion would be the element chlorine, if it could part with its electron.

During concentration, as the water evaporates, the ions of sodium and chlorine are brought nearer each other, and they combine to form solid salt when enough water has been removed. But even when combined to form salt in the solid state, the electron does not leave the chlorion and attach itself to the sodion; if that happened the result would be metallic sodium and chlorine gas; and they are certainly not formed. A crystal of salt differs from a solution of salt in much the same respects as a piece of ice differs from water; the one is solid and the other is liquid; but evidently the same stuff is there; the only difference is in the solidification.

It must therefore be supposed as a legitimate inference that when a lump of sodium unites with chlorine and burns in it as a lump of coal burns in air, the act of combination consists of the transference of an electron from the sodium metal to the chlorine; the result of this transference is to convert the sodium metal into sodions

and the chlorine gas into chlorions. These are substances with quite different physical and chemical properties from the metal sodium and the gas chlorine.

On dissolving in a little water, some of the chlorions and sodions, but only a few, become separated; however, if water be added so as to dilute the solution, a larger and larger number separate, until at a sufficient dilution all are separated. In fact, if this conception be extended, all chemical combinations should be regarded as the transference of electrons from one set of elements to another.

But not all compounds are split into ions when they are dissolved; it may be conjectured that in the case for instance of such a compound as sugar, which dissolves in water as such, the atoms of carbon, hydrogen, and oxygen, of which it consists, have interchanged electrons, otherwise chemical combination would not exist; but that the ions do not part from each other, even when opportunity is given by dissolving the sugar in water.

Although facilities for motion in many cases lead to separation of ions, it does not follow that when facilities are present separation will always take place.

When common salt is melted, which takes place if it be heated to redness, the ions separate; that this is the case is proved by its being then able to conduct electricity. Melted glass is also a conductor, although solid glass is not; and the reason again is probably in the fact that the ions have no freedom of motion in the solid.

These considerations, however, though closely connected with the nature of ions, are not in such close touch with the subject of this essay, the motive power of electricity. Perhaps a last analogy may make the explanation which I have tried to give somewhat clearer; it is this:

Place a dilute solution of salt in one vessel and a concentrated solution in another; cover both vessels with

a bell jar; pump out all air, so that the bell jar is filled only with vapour of water, and leave the whole standing for a long time. The weak solution will grow stronger, for it will evaporate; and the strong solution will grow weaker, for the vapour of water will condense in it. Now imagine that the two salt solutions are placed, not under the same bell jar, but under two separate bell jars, and that these bell jars are connected by a pipe. In the middle of this pipe is a little engine; the pipe from the weak solution enters the steam pipe of the cylinder, and the pipe leading from the cylinder, which would in an ordinary engine lead to the exhaust, is connected with the bell jar containing the stronger salt solution; then, if the engine is delicate enough, it will be driven by the current of vapour passing from the weak salt solution to the strong one.

Why? Because although steam can pass away from the surface of the water, salt cannot; the surface of the water is a diaphragm which will allow steam to pass, but which is impenetrable for salt.

The analogy with a battery is this: The zinc plate is like the weak solution of salt; when it dissolves, it gives up electrons at its surface; these electrons can pass along the wire, which is the analogue of the steam-pipe; if required, a small magneto-electric engine could be interposed so that it would be driven by the current passing through the wire, that is, by the stream of electrons, just as the steam-engine is driven by the current of steam.

On arriving at the copper plate the electrons combine with hydrogen ions and escape; and in this respect the battery described resembles rather a high pressure engine. But if desired the electrons may be kept in the system; it is only necessary to surround the copper plate with some substance such as sulphate of copper, and the electrons

are retained by uniting with the copper ions, when copper atoms will be deposited on the copper plate.

Just as the surface of the water forms a diaphragm through which salt cannot pass, while steam can, so the surface of the zinc plate forms a diaphragm through which matter such as zinc, hydrogen, or chlorine ions cannot pass, while electrons can, and they are also able to be conveyed by the wire, as steam is conveyed through the pipe. The motive power both of steam and electricity, in a word, is due to their passing from a region where their pressure is high to where it is low.

THE AURORA BOREALIS

THE Northern Lights, or the Merry Dancers, as they are often called, must have attracted attention in our country ever since it was inhabited. But whether owing to their frequent appearance they escaped chronicling, or whether records of natural phenomena were regarded as unimportant, I can find no mention of them in Scottish records. South of the border and across the English Channel mention is occasionally made of them; for in these more southern regions their occurrence was sufficiently uncommon for the display to attract attention. They were often supposed to portend disaster. An account of an aurora seen in London in 1560 likens it to 'burning spears':—

> 'Fierce fiery warriors fight upon the clouds,
> In ranks and squadrons and right form of war.'

An aurora was described by Cornelius Génune, Professor at Louvain, in 1575; several were seen by Michael Mestlin, tutor to the famous Kepler, in 1580; and in April and September 1581, and in September 1621, brilliant auroras were chronicled. From that date until 1707 there is no mention of an aurora having been seen.

It has long been known that the compass-needle, which usually points northward, and is inclined at an angle to the horizon (or is said to 'dip'), becomes disturbed and oscillates when an aurora is seen in the sky. It was the celebrated Halley[1] who, in 1714, hazarded the bold conjecture that the aurora was therefore a magnetic pheno-

[1] *Philosophical Transactions*, xxix. No. 341.

menon; the oscillations of the compass may even exceed ten minutes of arc, as observed by Mr. James Glaisher[1] in 1847. And many hypotheses have been brought forward to account for the connection between the two simultaneous phenomena. The last few years have seen the equipment of expeditions to Iceland, Finland, and Northern America, which have had for their principal object the observation of the earth's magnetic disturbances and the corresponding auroral displays. Many theories have been advanced, and it will be my task to try to bring them before you, and to supplement them where they appear to be wanting.

Let us first, however, listen to an eloquent description of the Northern Lights from the pen of the celebrated Alexander von Humboldt:[2]—

'Low down in the distant horizon, about the part of the heavens which is intersected by the magnetic meridian (*i.e.* the point to which the compass-needle is directed), the sky, which was previously clear, is at once overcast. A dense wall or bank of cloud seems to rise higher and higher, until it attains an elevation of 8 or 10 degrees. The colour of the dark segment passes into brown or violet, and stars are visible through the smoky stratum, as when a dense smoke darkens the sky. A broad, brightly luminous arch, first white, then yellow, encircles the dark segment. . . . The luminous arch remains sometimes for hours together, flashing and kindling in ever-varying undulations before rays and streamers emanate from it and shoot up to the zenith. The more intense the discharge of the northern light, the more bright is the play of colours, through all the varying gradations from violet and bluish-white to green and crimson. The magnetic columns of flame rise either singly from the luminous

[1] *Philosophical Transactions*, lviii.

[2] *Cosmos* (Bohn's edit.), vol. i. p. 189.

arch, blended with black rays similar to thick smoke, or simultaneously in many opposite points of the horizon, uniting together to form a flickering sea of flame, whose brilliant beauty admits of no adequate description, as the luminous waves are every moment assuming new and varying forms. Round the point in the vault of heaven which corresponds to the direction of the inclination of the needle, the beams unite together to form the corona—the crown of the northern light—which encircles the summit of the heavenly canopy with a milder radiance and unflickering emanations of light. It is only in rare instances that a perfect crown or circle is formed; but, on its completion, the phenomenon has invariably reached its maximum, and the radiations become less frequent, shorter, and more colourless. The crown and the luminous arches break up, and the whole vault of heaven becomes covered with irregularly scattered broad, faint, almost ashy grey, luminous, immovable patches, which in their turn disappear, leaving nothing but a trace of the dark smoke-like segment on the horizon. There often remains nothing of the whole spectacle but a white, delicate cloud, with feathery edges, or divided at equal distances into small roundish groups, like cirro-cumuli.'

These phenomena are also visible in the Southern hemisphere, and are produced by the Aurora Australis.

The luminous arches were also well described by Mr. William Key in a letter to Dr. Priestley, published in the *Philosophical Transactions* for 1783. He noticed that the summit of the arch passed near or through the pole-star; the arches were not always accompanied by the 'dancers.' Key, following Canton unwittingly, connected the aurora with discharges of electricity through rarefied gases. His words are: 'Let me hazard a conjecture respecting the white colour and stationary appearance of some of these arches. Experiments in electricity, made

with what is called an 'exhausted' receiver, show that th colour and motion of the electric spark vary in proportio to the rarity of the air in the receiver. The more the ai is rarefied, the more movable and coloured is the electri *aura* passing through it. On the contrary, the colour o the spark approaches to whiteness, and moves with greate difficulty, as the air is admitted. Will this observatio serve in any measure to account for the difference i colour and motion of these electrical arches, for such I presume to call them? May we not suppose the mor coloured and brilliant portions of the aurora borealis to b made in the rarer parts of the atmosphere, while the mor white and stationary ones possess the denser parts? Th whitest arches which I saw were the most fixed.'

Repeated attempts have been made to ascertain th height of an aurora. Henry Cavendish, the celebrate chemist, calculated the height from the data furnished b three observers of an aurora which was seen in 1784—on of whom was stationed at Cambridge, one at Kimbolto in Huntingdonshire, and one at Blockley in Gloucester-shire—by simple trigonometry, knowing the angle whic the summit of the arch appeared to subtend with th horizon in each case; the results are very reasonably con-cordant, and give an altitude of 52 to 71 miles. Man subsequent attempts have been made, and with simila results. One of the latest writers, Professor Birkeland of Christiania, gives as limits 62 to 124 miles (100-20 kilometres). The difficulty in such observations is t make sure that the observers in different places hav been measuring the same arch at the same moment. I shall have occasion later to bring evidence of a totall different character to confirm the general accuracy of suc measurements.

Between the years 1786 and 1793 John Dalton, wh was as indefatigable a meteorologist as he was a distin-

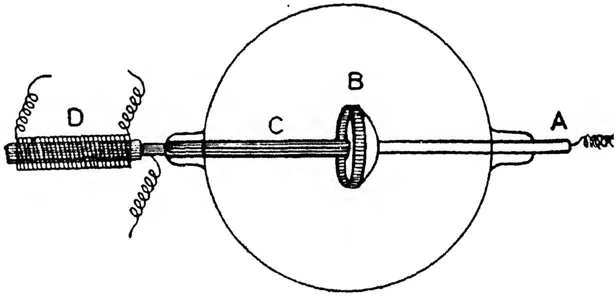
guished chemist, observed, from Kendal and Keswick, in Cumberland, no fewer than 250 displays of northern lights; he established the fact that the highest part of the luminous arc lies exactly above the magnetic pole, and that the streamers are parallel, at least 'over a moderate extent of country,' with the compass-needle as it dips towards the magnetic pole, which is believed to exist in the north of Canada, within the Arctic circle.

The celebrated De la Rive, of Geneva, made an attempt to reproduce the aurora in the interior of a glass vessel.[1] He started from the fact that the atmosphere is always charged with positive electricity, and that the earth is negatively electrified; he presumed, accordingly, that the two kinds of electricity would neutralise one another, and that currents would, as a rule, rise vertically to the earth's surface. Neutralisation occurs slowly when rain or snow falls, and suddenly when lightning flashes. De la Rive's theory is that in the upper regions of the atmosphere electric currents circulate from the equator to the two poles; and terrestrial currents, in the interior of the earth, are continually flowing from the poles towards the equator. Conduction, he thought, would be easier in the higher than in the lower regions of the atmosphere, and also better at the poles than near the equator, because of the moisture and frequent mists in the polar atmosphere. The discharge through the polar air would, he believed, render the mist luminous, and thus produce the phenomena already described.

His apparatus, which I had the good fortune to see in September 1902 at Geneva, consisted of a globe with a neck at each 'pole.' Through one of these necks passed a copper rod *A*, one end of which was connected with the positive discharge of an electrical machine; at the other end a ring of copper *B* was attached. *C* is an insulated

[1] *Mémoires de la Soc. de Phys. et d'His. Nat. de Genève*, vol. xiii.

soft-iron bar covered with an insulating compositio projecting through the opposite neck of the flask touching the exposed end of this rod with an ele



DE LA RIVE'S APPARATUS FOR REPRODUCING THE AURORA

magnet *D*, it also became a magnet. The air in the g having been rarefied, a brush discharge took place bet the ring *B* and the end of the rod *C*; on making magnet, the discharge became more luminous and reg and revolved round the magnet, sending streamers tow the end of the rod. In De la Rive's model the co ring represents the atmosphere, from which electr discharges to the rod *C*, representing the earth. He cluded that the aurora forms a luminous ring round magnetic poles as centres, with a greater or less diam the ring rotating on account of the earth's magnet that it is due to a discharge from the positively electr atmosphere to the negatively electrified earth, the sep tion of the two kinds of electricity being caused by action of the sun, chiefly in equatorial regions; discharges are of constant occurrence, though with varying intensities, and that the cirro-cumulus clouds also illuminated by such discharges.

When the spectroscope was turned on the aurora presence of a green line of wave-length about 5570 u

was noticed. Different observers gave:—Ångström, 5568; Vogel, 5572; Vijkander, 5573; Lemström, 5570; Huggins, 5570·4; Copeland, 5573; Gyllenskjöld, 5569; Campbell, 5571·6; Sykera, 5570; the Danish Mission to Iceland in 1899-1900, 5570. Many other lines have been photographed, of which more hereafter; but this line is extraordinarily intense, and, indeed, can often be seen when there is no visible aurora by simply directing a pocket spectroscope towards the north. The line when first observed was not known to be characteristic of the spectrum of any element.

In 1898 I had the honour to announce to the Royal Society the discovery, in conjunction with my assistant, Dr. M. W. Travers, of the existence of three new elementary substances in the atmosphere, to which we gave the names—neon, or 'the new one'; krypton, or 'the hidden one'; and xenon, or 'the stranger.'

The spectrum of neon is characterised by many lines in the red, orange, and yellow; while that of xenon shows many green and blue lines. The light evolved from tubes containing these gases under low pressure when an electric current of high tension is passed through them, is of a corresponding hue; thus neon sends out a splendid rose or flame-coloured light; and xenon, a sky-blue; while the light of krypton is nearly white, although seen by some of a pale lilac, and by others of a pale-green colour.

The densities of the elements proved to be as had been expected: that of neon, compared with hydrogen taken as 2, is 20; of krypton, 82; and of xenon, 128.

Shortly after the discovery of krypton, my assistant, Mr. Baly, measured carefully the wave-lengths of its more important lines; and one of these, a very brilliant green line, had the wave-length 5570·5. The day after this was published, Sir William Huggins wrote me privately

pointing out the identity of this wave-length with the principal auroral line; and a week later, Professor Arthur Schuster, in a letter to *Nature*, called attention to the same coincidence.

It therefore appeared probable that the aurora might be produced by electric discharges in the upper atmosphere, through a gas in which krypton was present in considerable amount.

In the meantime Professor Paulsen, of Copenhagen, has been examining the photographed spectra of the aurora collected by the Icelandic and Finland expeditions; and it appears probable that many of the lines seen in the auroral spectrum are identical with those of the common gas nitrogen, seen at the cathode terminal of a vacuum-tube. Professor Paulsen has had the great kindness to send me prints of two photographs, the lines of which are numbered I. and II. in the following table. I have added in a parallel column the corresponding wave-lengths of krypton lines.

	Lines of auroral and of cathode nitrogen spectrum.		Lines of krypton.	Intensity.
	I.	II.		
1	Absent.	3140	—	—
2	"	3160	—	—
3	"	3370	—	—
4	Absent.	3540	—	—
5	3580	3580	3590	7
6	3710	Absent.	3718	10
			3719	8
			3721	7
7	3760	3760	3754	5
8	3800	3800	3805	4
9	3920	3920	3920	8
10	4000	4000	3995	6
			3998	5

	Lines of auroral and cathode nitrogen spectrum. I.	II.	Lines of krypton.	Intensity.
11	4060	4060	4057	8
12	4260	4260	4274	8
13	5570	Absent.	5570	10

The last is *the* characteristic line of the aurora, and is one of the two brilliant lines of the krypton spectrum; the other brilliant krypton line is in the yellow, and cannot be easily photographed when the light is not bright but flickering, as the auroral light is.

I am not able to decide yet whether the lines are all due to krypton or to the cathode spectrum of nitrogen. Certainly there is a striking similarity between the nitrogen spectrum and that of the aurora; and, on the other hand, the lines of krypton, though sufficiently coincident with those of the aurora to satisfy criticism, leave other bright lines of the krypton spectrum unaccounted for. Yet the cathode spectrum of nitrogen does not contain the line 5570, the most brilliant of the auroral spectrum, and the one most easily discovered by aid of a pocket spectroscope. Experiments on this matter are not yet decisive.

Moreover, it appears improbable that the aurora should always exhibit only one spectrum. The discharge of electricity through a mixture of gases reveals more or less completely the spectrum of each. Those gases which are present in smallest amount have, as a rule, their spectra proportionately enfeebled. But it does not always happen that all the lines of the spectrum of any one gas are proportionately enfeebled; sometimes the character of the spectrum itself is altered. The interposition of a Leiden jar and a spark-gap often causes a radical alteration in the spectrum of a gas. This can be well seen with argon; when the discharge is altered, many of the red lines of the

spectrum disappear and blue lines become visible; hence the colour of the discharge changes from red to blue. And other gases exhibit the same kind of change, though not generally in so striking a manner. Further, it has been shown by my colleague, Dr. Collie, that an element may develop a new and strong line in its spectrum on being mixed with a certain gas, which it does not exhibit if another gas be substituted. All these questions are very obscure, and have not as yet been investigated; only the fringe of the subject has been touched. As the aurora, without doubt, is visible on different occasions at very different altitudes, it is more than possible that the spectra will differ. The appearance of red auroras would imply a spectrum in which red lines predominate; but I am not aware of any observation having been made of the spectrum of a red aurora. From the similarity of colour it might well be conjectured that the red tint is due to the discharge occurring through an atmosphere comparatively rich in neon.

Assuming for the moment the identity of the line of wave-length 5570 with that of krypton, two questions at once suggest themselves. First, why should this line be so remarkably brilliant when krypton is present in the atmosphere in comparatively very minute amount? What are the relative intensities of the spectra of krypton and of other gases under similar circumstances? And second, is there any process which will tend to increase the relative amount of krypton in the upper regions of the atmosphere? I have attempted to answer both these questions.

Some years ago, in conjunction with Professor Collie, experiments were made on the visibility of the spectrum of one gas in presence of another with which it was diluted.[1] The results are given in the following table:—

[1] *Proc. Roy. Soc.*, lix. 257.

AMOUNT OF GAS DETECTABLE IN A MIXTURE

1.	Helium in hydrogen,	10 per cent.	of helium barely visible.
2.	Hydrogen in helium,	0·001 „	of hydrogen visible.
3.	Nitrogen in helium,	0·01 „	of nitrogen almost invisible.
4.	Helium in nitrogen,	10 „	of helium difficult to detect.
5.	Argon in helium,	0·06 „	still visible.
6.	Helium in argon,	25 „	invisible.
7.	Nitrogen in argon,	0·08 „	just visible.
8.	Argon in nitrogen,	37 „	barely visible.
9.	Argon in oxygen,	2·3 „	difficult to distinguish.

This table shows the enormous differences which exist between the behaviour of different gases. To take the extreme cases—while it is possible to detect 1 part of hydrogen in 100,000 of helium, it is barely possible to recognise 1 part of argon in 2 of nitrogen.

Similar experiments with krypton showed that

In air,	1 part of krypton is visible in	7,100	parts.
In oxygen,	1 „ „ „	1,250,000	„
In hydrogen,	1 „ „ „	67	„
In argon,	1 „ „ „	7,150	„
In helium,	1 „ „ „	2,860,000	„

The pressure of krypton, too, in the case of air is almost inconceivably low; it amounts to only one thirty-millionth of the usual atmospheric pressure. This shows the enormous persistency of the krypton spectrum—that is, of the most conspicuous line, the auroral green, for that was the one observed in all instances. If, then, an electric discharge passes through the upper and rarefied strata of the atmosphere, the probability of detecting the green line of krypton will be much greater than that of detecting the spectrum of any other element, even though the latter be present in enormously greater proportion. Hydrogen alone has any marked power of extinguishing the spectrum of krypton.

It is possible to calculate the maximum height of the aurora, on the supposition that the krypton line is no

longer visible when the pressure falls below 0·0000 millimetre—the pressure observed when, in a mixture krypton and helium, the green line of krypton becar very faint and almost invisible. Neglecting the influen of temperature, the pressure of the atmosphere can made to give its height by the formula—

$$H = 18{\cdot}382\ (\log.\ B - \log.\ b)\ \text{kilometres.}$$

Substituting for B (barometer) its normal height, 7 millimetres, and for b the pressure of the krypton, 0·0000 millimetre, we have height = 135 kilometres, or about miles. This number is reasonably near the figures giv by Cavendish and others. Professor Birkeland, the late authority, it will be remembered, thinks the altitude from 100 to 200 kilometres,[1] or from 62·5 to 125 miles.

We may next ask—Since the spectrum of krypton so persistent, why is it not visible in air? The answer —Because the presence of nitrogen renders it invisib for it is not possible to distinguish less than one part krypton by volume in 7100 parts of air. But krypt does not show its spectrum in argon, which may be sa to constitute about 1 per cent. of the volume of air. N 7100 parts of air will yield about 70 parts of argon, and should be possible to distinguish the krypton line if t amount of krypton present were 0·01 part, or 1 part 7000. This would give, for the proportion of krypt in air, 1 part in 700,000. Recent experiments, ha shown that it is possible to extract 1 part of krypt from about 7,000,000 of air; and of xenon, which, owi to its lower vapour pressure, can be extracted from with more ease than krypton, there is only about 1 pa in 40,000,000 of air. It is therefore clear why the spe trum of krypton is not visible in that of crude argon.

We come next to the question—Is there any reas to believe that krypton may concentrate in the high

[1] *Expédition Norvègienne*, Christiania, 1901, p. 28.

regions of the atmosphere, that is, that its proportion, relatively to that of the more abundant gases oxygen and nitrogen, may increase as the altitude grows greater? To this, I think, an affirmative answer may be given. Let us consider the grounds for the supposition.

When a gas is compressed it turns warm, as every one knows who has used a bicycle-pump. Conversely, when it escapes from compression it cools itself. But all gases do not heat or cool equally for equal amounts of compression or expansion; for some gases are raised to a higher temperature than others by absorbing the same quantity of heat; and the same quantity of heat is, practically, generated by the same degree of compression, or absorbed by the same degree of expansion; for work is quantitatively equivalent to heat, as was shown by Joule half a century ago. Now 40 grams of argon should, if the specific heat of that gas were the same as that of oxygen, require the same amount of heat to raise its temperature through 1 degree; or put in another way, if each of these gases were expanded to the same amount, they would be equally cooled, if equal amounts of heat were requisite to raise their temperatures through an equal number of degrees. But this is not the case. Argon requires less heat to raise its temperature than oxygen, in the ratio of 3 to 5 if it is not allowed to expand, or in the ratio of 5 to 7 if expansion is possible under constant pressure. If allowed to expand under circumstances in which volume increases while pressure falls, some ratio intermediate between these would show the difference in cooling; the exact amount depending on the degree of expansion in question. Broadly stated, argon, in expanding will cool itself considerably more than oxygen or nitrogen; while its congeners, helium, neon, krypton, and xenon, will exhibit a degree of cooling practically identical with that of argon.

The next point to be considered is that gases diffuse

freely into one another when left in contact; so that a heavy gas will mix readily with a much lighter one, even though the heavy one may be below and the lighter one above. This diffusion results from the motion of the molecules of gases; and as the rate of motion depends on the temperature of the gas, those molecules which happen to have a high temperature move much more rapidly than those with a lower. When two gases mix, however, or when a hot gas mixes with a cold one, the more rapidly moving, and therefore hot, molecules very rapidly communicate their motion to the colder gas, raising its temperature until the temperatures of both sets of molecules are equalised. This is due to the enormous number of encounters which take place between the molecules, partly on account of the minute size of each molecule, and the consequent number in even a very small volume; and partly to the great rate at which they are moving. For example, it can be calculated that in all probability there are 50,000,000,000,000,000 or 50 quadrillion molecules of hydrogen in a cubic millimetre of that gas (about the volume of the head of a large pin) and that the average velocity of each molecule is at the rate of 4½ miles per second. No wonder, therefore, that the exchange between molecules of different temperature is almost instantaneous. It must nevertheless be borne in mind that hot gases diffuse much more rapidly than cold ones.

The densities of the gases constituting the atmosphere are as follows, the standard being that of oxygen taken as 16:—

		$1/\sqrt{d}$			$1/\sqrt{d}$
Water Vapour,	9	0·333	Neon,	10	0·316
Nitrogen,	14	0·287	Argon,	20	0·224
Oxygen,	16	0·250	Krypton,	41	0·156
Carbon Dioxide,	22	0·213	Xenon,	64	0·125
Helium,	2	0·707			

The relative rates of diffusion are inversely as the square roots of the densities, and are given in the second column. For example, oxygen escapes into a neighbouring layer twice as quickly as xenon; and helium nearly three times as fast as oxygen. Now it is evident that the gases which will escape most slowly are krypton and xenon; carbon dioxide and argon come next in order; while nitrogen, neon, water vapour, and helium escape more rapidly in the order given. If then a jar with porous walls were full of air, and were exposed to some indifferent atmosphere, the gas remaining in the jar after some time would contain more of the heavier and less of thelighter gases proportionally to the original amounts present.

The third premiss in the argument is that in equatorial regions there is an upward current of air, due to the warming of the earth by the nearly vertical rays of the sun and the consequent expansion of the air in contact with the soil or the sea; while in the polar regions there is a continual downward current, produced by the cooling of the air in contact with the ice of the polar caps. This circulation of the atmosphere was investigated by Professor James Thomson in 1857, and his Bakerian lecture on the subject appeared in the *Philosophical Transactions* for 1892, p. 653. The conclusion to which he came is that the upward atmospheric current at the equator on reaching the higher regions of the atmosphere, divides into two, and while one part of the air travels in a north-easterly direction, the other half travels in a south-easterly direction towards the north and south poles respectively. Arrived at the neighbourhood of the polar caps, the air descends and, broadly stated, travels back near the surface of the earth again towards the equator. We need not here regard eddies which occur near the tropics of Cancer and Capricorn; the main features are sufficient for our purpose.

The air, then, as it ascends from equatorial latitudes cools itself in the process; and from what has been said, it would seem that gases of the argon group would cool more rapidly than the other atmospheric gases, oxygen and nitrogen. *Pour préciser les idées*, as the French say, let us confine our attention to the northern hemisphere, and let us suppose that a vertical partition has been set up in the neighbourhood of the equator, quite permeable to gases, and surrounding the earth much as the wooden frame of a terrestrial globe surrounds the globe. Indeed, if we conceive of that frame as a double one, and the ascending current rising between the walls, we shall realise what is intended. As the upward current gains in height, it falls in temperature; and during the whole of the ascent the nitrogen and oxygen are passing through the porous diaphragm at a rate greater than that of the argon gases. With increasing height the density of all gases decreases; their molecules are more widely separated from each other, and interchange of velocity or, what is equivalent, interchange of temperature becomes less rapid; hence the separation should be a more perfect one the greater the altitude. But, at the same time, the argon gases are not wholly left in the upward current; many molecules will pass the barrier; why should they not return in as great number as they pass? Because after passing the partition they no longer move upwards with the same velocity as before; the farther they progress towards the north the less inducement to rise, for the temperature of the earth is lower.

This reasoning is equally applicable if we regard the barrier as removed; we may mentally surround the earth with an infinity of such barrier rings, parallel to the plane of the equator; and it will still remain true that the warmer gases will tend to escape in the lower regions of the atmosphere, leaving the cooler gases to ascend.

At the poles the process is reversed. The argon g
are more heated during their descent than the oxygen
nitrogen, and will escape into neighbouring layers
atmosphere at a greater altitude, on the average, than
latter. The relative rates of diffusion of these gases,
may play an even more important part in effecting
separation; the heaviest—argon, carbon dioxide, kryp
and xenon—will remain in the ascending layer in la
relative proportion than the oxygen, nitrogen, and o
lighter gases, and will, therefore, be carried to the u
regions of the atmosphere by the ascending equato
current; but in the descending polar current the pro
would be reversed, for the heavier gases would be car
down in the current with less escape than the lig
ones. The effect of diffusion alone, neglecting the hea
or cooling of the gases, would, therefore, be neutral
during each complete circulation.

But the process of separation which depends on
difference of temperature of the gases, were there only
circulation, would in all probability be productive of
small result; it is to be observed, however, that the ef
is cumulative, that the process of concentration of
argon gases in the higher regions of the atmosphere g
on from age to age, and that there may now be
apparently stationary state, when separation of ar
gases from oxygen and nitrogen balances mixture
diffusion and the mixing which inevitably accompa
winds. It may be suggested that the greater freque
of auroras during years when the sunspots are large
easily visible may be connected with the higher tempe
ture of such years, as well as with the magnetic distu
ances which invariably accompany sunspots.

To sum up:—1. The gases of the argon group are m
easily heated and cooled than are oxygen and nitrog
2. They are cooled more than the latter during th

upward ascent at the equator, and therefore tend to concentrate in the ascending current as it reaches the confines of the atmosphere, owing to the more rapid escape of oxygen and nitrogen by diffusion. 3. The phenomena are reversed in the descending currents at the poles, and the argon gases tend to mix with neighbouring layers of gas at high altitudes. 4. The higher strata of the atmosphere are probably richer in the inactive gases than the lower strata.

As electric discharges producing the aurora certainly occur at great altitudes, the spectra seen are those of the inactive gases; and owing to the fact that the krypton green line, of wave-length 5570 units, is remarkably easily visible, even in an admixture of other gases, it happens to be the most conspicuous line in the auroral spectrum.

This leads us to consider, in the last place, the cause of the electric current. And here we enter on a different region of thought.

There are two theories on the subject, one due to Professor Birkeland,[1] of Christiania, the other to Professor Arrhenius,[2] of Stockholm. It has long been known that violet light rays and the invisible rays of the spectrum beyond the violet, which can be detected by photography, have the property of discharging a negatively electrified body. It is suggested by Professor Birkeland that the spots on the sun are caused by solar eruptions, or, to use a familiar word, volcanoes; and that the sun then emits negatively charged corpuscles, similar to those which are believed to constitute, partly at least, the cathode rays—rays producing those utilised for surgical practice in taking photographs of bones. Birkeland supposes that such corpuscles are 'sucked in' to the earth's magnetic poles, giving rise to vortices of electric currents in the upper

[1] *Archives des Sciences Phys. et Nat. de Genève*, June 1896.
[2] *Physikalische Zeitschrift*, ii. Nos. 6 and 7.

regions of the atmosphere. It is indeed known that such rays are deviated by the neighbourhood of a magnet; and also that the presence of large solar spots is always accompanied by magnetic 'storms' on the earth and the appearance of frequent and brilliant auroras.

The theory of Arrhenius is that the corpuscles emitted by the sun are not inconceivably minute bodies, but have an appreciable size; that they are, say, the thousandth of a millimetre, or the 25,000th of an inch in diameter, and that they are expelled from the sun by the repulsive action of light.

Whichever theory be correct, it is probable that negatively electrified gaseous molecules are present in the upper regions of the atmosphere, and it is also probable that these molecules receive their charge most readily where they are most exposed to a vertical sun, that is, at and near the equator. We have seen that Professor James Thomson's upper aerial currents would carry these and other molecules towards the poles; they would move spirally northwards and southwards with an easterly trend. As they approach the poles their number per unit area will obviously increase; for the terrestrial parallels of latitude decrease in circumference the nearer they are to the poles. It is to be expected that before the actual poles are reached, the potential of the upper air should increase to such an extent as to produce a luminous discharge, in the form of a ring or halo, with the magnetic poles as their centres. It is conceivably this ring which we see as an arch in the sky; it may not be so high as the coloured streamers, and may well give the nitrogen spectrum. It must be remembered, however, that the earth is a huge magnet; and that lines of force connect the poles in a fashion shown in the figure. The halo, exposed to these magnetic forces, will send out streamers towards the poles as well as towards the zenith; as they approach the

equator, however, the light will fade, owing to the spreading and weakening of these lines.

An imperfect attempt has been made to imitate the auroral phenomena, but it nevertheless shows in some degree the appearance of the northern lights. A globe, containing krypton at very low pressure, is suspended between the poles of a powerful electro-magnet;[1] a ring, consisting of five or six coils of covered wire, is laid on the top of the globe, and by help of an induction-coil and a Leiden jar, strong induction discharges are made to pass through the coil. Each discharge is accompanied by a circular discharge in the interior of the globe. The effect is that of a ring or halo near the upper side of the jar. On 'making' the electro-magnet, the ring sends out streamers, exactly similar in appearance to auroral streamers, and like them they have a rotary motion and flicker, shortening and lengthening, just as natural streamers do. If it were possible to place a magnetic model of the earth inside such a globe, I doubt not that the streamers would follow directions similar to those in the figure, and that the imitation would still more closely resemble the reality. The light evolved from pure krypton, under the influence of such discharges, is of a whitish steel-blue colour with occasional green and lilac flickers, and it also recalls the appearance of the natural aurora. But, as already remarked, it is more than probable that the spectrum of the aurora, seen at different times and in different altitudes, may show not only the spectrum of krypton, but also those of the other atmospheric gases.

One more remark before concluding. The temperature of the upper atmosphere is undoubtedly very low; but at such altitudes even xenon, the least volatile of the atmospheric gases, possesses so high a vapour pressure that it

[1] The electro-magnet belonging to a small 1 h.p. dynamo was used.

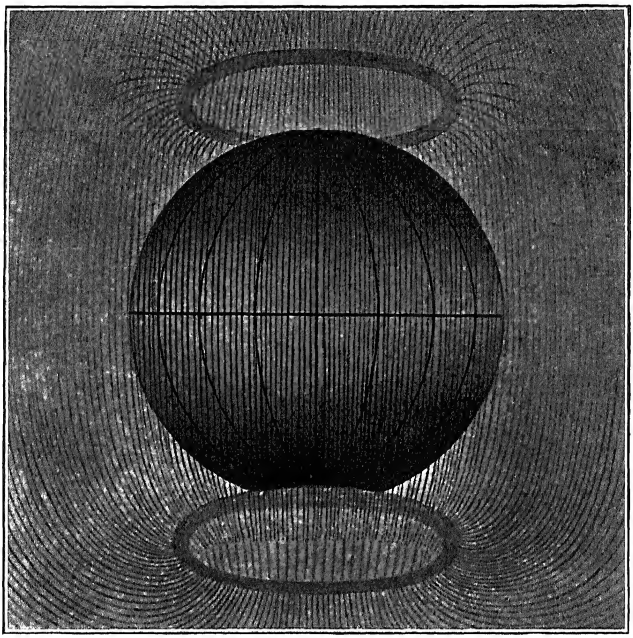


DIAGRAM OF THE EARTH AND THE POLAR AURORAS

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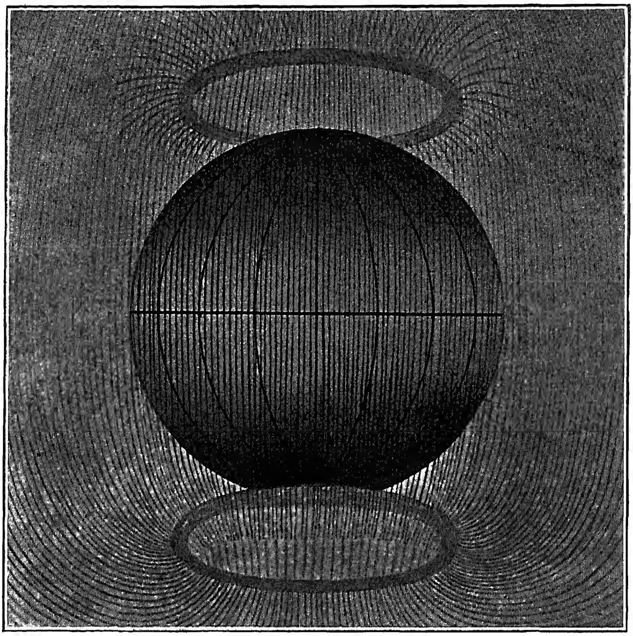

DIAGRAM OF THE EARTH AND THE POLAR AURORAS

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would certainly remain gaseous; for in order to liquefy or solidify a gas, not merely reduction of temperature, but also considerable pressure, is required. It is, however, quite possible that water-vapour, in the lower regions frequented by the aurora, may exist in a supersaturated condition, and that the electric discharges may bring about condensation, and on the large scale of nature, as on the small scale of a laboratory experiment, produce mists, and so give rise to the cirro-cumulus clouds which so often accompany an aurora.

The solving of conundrums has for many people a great attraction; Nature surrounds us with conundrums, and it is one of the greatest pleasures in life to attempt their solution. Whether or not I have been successful in offering a partial solution of the one which we may call the 'Merry Dancers,' time will show.

THE FUNCTIONS OF A UNIVERSITY

ORATION DELIVERED AT UNIVERSITY COLLEGE, LONDON
JUNE 6, 1901

I AM about to speak of the Functions of a University. The word University has borne many significations; and, indeed, its functions are various, and the signification attached to the word has depended on the particular point of view taken at the time. An eminent German, who visited me some years ago, made the remark after seeing University College:—'Aber, lieber Herr College, University College ist eine kleine Universität.' So it is; for it fulfils most of the functions of the most successful Universities in the world. A countryman of the gifted founder of this College, Thomas Campbell, a man who has left even a deeper mark than he on the literature of the world, said:—

'O wad some Pow'r the giftie gie us
To see oursels as ithers see us!'

Were that gift given us, I am confident that we should have no cause to blush. One of the most necessary conditions of success is confidence in oneself—'a gude consait of oursels,' as the Scots saying has it; and I know that learned men throughout the world look on the work done at University College as among the best produced. And why is this? Because the traditions of University College have always been that it is not merely a place where known facts and theories should be ad-

ministered in daily doses to young men and young women, but that the duty of the professors, assistant-professors, teachers, and advanced students is to increase knowledge. That is the chief function of a University—to increase knowledge. But it is not the only one.

A University has always been regarded as a training school for the 'learned professions,' *i.e.* for Theology, Law, and Medicine. The terms of our charter have excluded the first of these branches of knowledge. Founded as it was in the 'twenties, when admission to Oxford or Cambridge involved either belief in the tenets of the Church of England, or insincerity, it was not possible to provide courses in Theology which should be acceptable to Nonconformists, Jews, and others who desired education. On the whole, it appears to me better that a subject, about which so much difference of opinion exists, should be taught in a separate institution. There are many branches of knowledge which can be adequately discussed without intruding into any sphere of religious controversy; and, indeed, it would be difficult, I imagine, to treat mathematics or chemistry from a sectarian standpoint. I, at least, have never tried. There are subjects which may be placed on the border-line, for example, Philosophy; but such subjects, and they are few in number, might well form part of the curriculum of the theological college, if thought desirable. It is a thousand pities that instead of founding King's College, a theological college had not been established in the immediate neighbourhood of University College; it would have strengthened us, and it would have tended, too, to the advantage of the Church of England. However, what is done can't be undone; and let us wish all prosperity to our sister college, and a long and useful life. We are now friends, and have been friends for many years. May that friendship long continue!

Dismissing the Faculty of Theology, therefore, as out of our power, as well as beyond our wishes, let us turn to the remaining two learned professions. University College, I believe, was the first place in England where a systematic legal education could be obtained. Our chairs of Roman Law, Constitutional Law, and Jurisprudence were the first to be established in England, although such chairs had for long been known on the Continent and in Scotland. 'Imitation is the sincerest flattery'; and in the fulness of time, the Inns of Court started a school of their own. Our classes, which used to be crowded, dwindled, and our law-school is certainly not our strongest feature. I am not sufficiently acquainted with English legal education to pronounce an opinion as to whether methods of training as they at present exist in England are the most effective: I have heard rumours that they are not. That must be left to specialists to decide. But arguing from the experience of another faculty, in which the apprenticeship system once existed, and which has changed that system with a view to reform, and judging, too, from experience abroad and in Scotland, I venture to think that some improvement in legal education is possible. If that opinion is correct, it is surely not too much to hope that the claims of University College may be considered as having made the first attempt to systematise legal education in England.

The Faculty of Medicine has existed in a flourishing state since the inception of University College. Not long after the College was built, the old Hospital buildings, were erected. One of my predecessors, on a similar occasion to this, has given you an entrancing account of the early history of this side of the College, and has discoursed on the eminent men who filled the chairs in the Medical Faculty. Here young men whose intention it is to enter the medical profession are trained; they now receive five

years' instruction in the various branches of knowledge bearing on their important calling. I would point out that this function of a University is professedly a technical one: the training of medical men. True, many researches have been made by the eminent men who have held chairs in this Faculty; but that is not the primary duty of such men; their duty is to train others to exercise a profession. If they advance their subject in doing so, so much the better; it increases the fame of the school, it imparts enthusiasm to their students, and in many cases their discoveries have been of unspeakable benefit to the human race. In a certain sense, every medical man is an investigator; the first essential is that he shall be able to make a correct diagnosis; the next, that he shall prescribe correct treatment. But novelty is not essential; few men evolve new surgical operations or introduce new remedies; and though we have in the past had not a few such, they are not essential for a successful medical school, the object of which is to train good practical working physicians and surgeons. The teaching staff of the Medical Faculty must of necessity be almost all engaged in practice, and, indeed, it would be unfortunate for their students if they were merely theoretical teachers. Let me recapitulate my point: the Medical Faculty is essentially a technical Faculty; the hospital is its workshop.

In England, of recent years, schools of engineering have been attached to the Universities. Abroad and in America they are separate establishments, and are sometimes attached to large engineering works, where the pupils pursue their theoretical and practical studies together, taking the former in the morning, the latter in the afternoon. Here again the subject is a professional one. The object of the student is to become a practical engineer, and all his work is necessarily directed to that end. Like other workers in different fields, his aim is the

acquisition and utilisation of 'power,' but in his case it is his object to direct mechanical and electrical power so as to add to the convenience of the public. A machine is an instrument for converting heat or electrical energy into what is termed 'kinetic energy,' and it is with the laws and modes of this conversion that he has to deal. Such abstract sciences as chemistry, physics, and geology, therefore, are studied as means to an end; not for their own sakes. They afford him a glimpse of the principles on which his engineering practice is based; and mathematics is essential in order that he may be able to apply physical principles to the practical problems of his profession.

We see, then, that a University, as it at present exists, provides, or may provide, technical instruction for theologians, for lawyers, for medical men, and for engineers. It is, in fact, an advanced technical school for these subjects.

But it is more, and I believe that its chief function lies in the kind of work which I shall attempt now to describe. The German Universities possess what they term a 'Philosophical Faculty'; and this phrase is to be accepted in the derivational meaning of the word — a faculty which loves wisdom or learning. The watchword of the members of this faculty is Research; the searching out the secrets of Nature, to use a current phrase; or the attempt to create new knowledge. The whole machinery of the Philosophical Faculty is devised to achieve this end; the selection of the teachers, the equipment of the laboratories and libraries, the awarding of the degrees.

What are the advantages of research? Much is heard nowadays regarding the necessity of state provision for its encouragement, and the Government places at the disposal of the Royal Society a sum of no less than £4000 a year, which is distributed in the form of grants to

applicants who are deemed suitable by committees appointed to consider their claims to assistance.

There are two views regarding the advantage of research which have been held. The first of these may be termed the utilitarian view. You all know the tale of the man of science who was asked the use of research, and who parried with the question—What is the use of a baby? Well, I imagine that one school of political economists would oppose the practice of child-murder on the ground that potentially valuable property was being destroyed. These persons would probably not be those who stood to the baby in a parental relation. Nor are the most successful investigators those who pursue their inquiries with the hope of profit, but for the love of them. It is, however, a good thing, I believe, that the *profanum vulgus* should hold the view that research is remunerative to the public—as some forms of it undoubtedly are.

The second view may be termed the philosophical one. It is one held by lovers of wisdom in all its various forms. It explains itself, for the human race is differentiated from the lower animals by the desire which it has to know 'why.' You may have noticed, as I have, that one of the first words uttered by that profound philosopher, a small child, is 'why?' Indeed it becomes wearisome by its iteration. We are the superiors of the brutes in that we can hand down our knowledge. It may be that some animals also seek for knowledge; but at best, it is of use to themselves alone; they cannot transmit it to their posterity, except possibly by way of hereditary faculties. We, on the contrary, can write and read; and this places us, if we like, in possession of the accumulated wisdom of the ages.

Now the most important function, I hold, of a University is to attempt to answer that question 'why?' The ancients tried to do so; but they had not learned

that its answer must be preceded by the answer to the question 'how?' and that in most cases—indeed in all—we must learn to be contented with the answer to 'how?' The better we can tell *how* things are, the more nearly shall we be able to say *why* they are.

Such a question is applicable to all kinds of subjects: to what our forerunners on this earth did; how they lived: if we go even further back, what preceded them on the earth. The history of these inquiries is the function of geology, palæontology, and palæontological botany; it is continued through archæology, Egyptian and Assyrian, Greek and Roman; it evolves into history, and lights are thrown on it by languages and philology; it dovetails with literature and economics. In all these, research is possible; and a University should be equipped for the successful prosecution of inquiries in all such branches.

Another class of inquiries relates to what we think and how we reason; and here we have philosophy and logic. A different branch of the same inquiry leads us to mathematics, which deals with spatial and numerical concepts of the human mind, geometry and algebra. By an easy transition we have the natural sciences; those less closely connected with ourselves as persons, but intimately related to our surroundings. Zoology and botany, anatomy, physiology and pathology deal with living organisms as structural machines; and they are based on physics and chemistry, which are themselves dependent on mathematics.

Such inquiries are worth making for their own sakes. They interest a large part of the human race; and not to feel interested in them is to lack intelligence. The man who is content to live from day to day, glad if each day will but produce him food to eat and a roof to sleep under, is but little removed from an uncivilised being. For the test of civilisation is *prevision*; care to look forward; to

provide for to-morrow; the morrow of the race, as well as the morrow of the individual; and he who looks furthest ahead is best able to cope with Nature, and to conquer her.

The investigation of the unknown is to gather experience from those who have lived before us, and to secure knowledge for ourselves and for those who will succeed us. I see, however, that I am insensibly taking a utilitarian view; I by no means wish to exclude it, but the chief purpose of research must be the acquisition of knowledge, and the second its utilisation.

I will try to explain why this is so, and here you must forgive me if I cite well-known and oft-quoted instances.

If attempts were made to discover only useful knowledge (and by useful I accept the vulgar definition of profitable, *i.e.* knowledge which can be directly transmuted into its money equivalent) these attempts would, in many, if not in most cases, fail of their object. I do not say that once a principle has been proved, and a practical application is to be made of it, that the working out of the details is not necessary. But that is best done by the practical man, be he the parson, the doctor, the engineer, the technical electrician, or the chemist, and best of all on a fairly large scale. If, however, the practical end be always kept in view, the chances are that there will be no advance in principles. Indeed, what we investigators wish to be able to do, and what in many cases we can do, although perhaps very imperfectly, is to prophesy, to foretell what a given combination of circumstances will produce. The desire is founded on a belief in the uniformity of Nature; on the conviction that what has been will again be, should the original conditions be reproduced. By studying the consequences of varying the conditions our knowledge is extended; indeed it is sometimes possible to go so far as to predict what will

happen under conditions, all of which have never before been seen to be present together.

When Faraday discovered the fact that if a magnet is made to approach a coil of wire, an electric current is induced in that wire, he made a discovery which at the time was of only scientific interest. That discovery has resulted in electric light, electric traction, and the utilisation of electricity as a motive power; the development of a means of transmitting energy, of which we have by no means seen the end; nay, we are even now only at its inception, so great must the advance in its utilisation ultimately become.

When Hofmann set Perkin as a young student to investigate the products of oxidation of the base aniline, produced by him from coal-tar, it would have been impossible to have predicted that one manufactory alone would possess nearly 400 large buildings and employ 5000 workmen, living in its own town of 25,000 inhabitants, all of which is devoted to the manufacture of colours from aniline and other coal-tar products. In this work alone at least 350 chemists are employed, most of whom have had a university training.

Schönbein, a Swiss schoolmaster, interested in chemistry, was struck by the action of nitric acid on paper and cotton. He would have been astounded if he had been told that his experiments would have resulted in the employment of his nitrocelluloses in colossal quantity for blasting, and for ordnance of all kinds, from the 90-ton gun to the fowling-piece.

But discoveries such as these, which lead directly to practical results, are yet far inferior in importance to others in which a general principle is involved. Joule and Robert Mayer, who proved the equivalence of heat and work, have had far more influence on succeeding ages than even the discoverers above mentioned, for they have

imbued a multitude of minds with a correct understanding of the nature of energy, and the possibility of converting it economically into that form in which it is most directly useful for the purpose in view. They have laid the basis of reasoning for *machines*; and it is on machines, instruments for converting unavailable into available energy, that the prosperity of the human race depends.

You will see from these instances that it is in reality 'philosophy' or a love of wisdom which, after all, is most to be sought after. Like virtue, it is its own reward; and as we all hope is the case with virtue too, it brings other rewards in its train; not, be it remarked, always to the philosopher, but to the race. Virtue, pursued with the direct object of gain, is a poor thing; indeed, it can hardly be termed virtue, if it is dimmed by a motive. So philosophy, if followed after for profit, loses its meaning.

But I have omitted to mention another motive which makes for research; it is a love of pleasure. I can conceive no pleasure greater than that of the poet—the maker—who wreathes beautiful thoughts with beautiful words; but next to this, I would place the pleasure of discovery, in whatever sphere it be made. It is a pleasure not merely to the discoverer, but to all who can follow the train of his reasoning. And after all, the pleasure of the human race, or of the thinking portion of it, counts for a good deal in this life of ours.

To return:—attempts at research, guided by purely utilitarian motives, generally fail in their object, or at least are not likely to be so productive as research without ulterior motive. I am strengthened in this conclusion by the verdict of an eminent German who has himself put the principle into practice; who after following out a purely theoretical line of experiment, which at first appeared remote from profit, has been rewarded by its

remunerative utilisation. He remarked, incidentally, that the professors in Polytechnika—(what we should term technical colleges, intended to prepare young men for the professions of engineering and technical chemistry)—were less known for their influence on industry than University professors. The aim is different in the two cases; the Polytechnika train men for a profession; the Philosophical Faculty of a German University aims at imparting a love of knowledge; and as a matter of fact the latter *pay* in their influence on the prosperity of the nation better than the former. And this brings me to the fundamental premiss of my Oration. It is this:—That the best preparation for success in any calling is the training of the student in methods of research. This should be the goal to be clearly kept in view by all teachers in the Philosophical Faculties of Universities. They should teach with this object:—to awaken in each of their students a love of his subject, and a consciousness that if he persevere, he, too, will be able to extend its bounds.

Of course it is necessary for the student to learn, so far as is possible, what has already been done. I would not urge that a young man should not master, or at all events study, a great deal of what has already been discovered, before he attempts to soar on his own wings. But there is all the difference in the world between the point of view of the student who reads in order to qualify for an examination, or to gain a prize or a scholarship, and the student who reads because he knows that thus he will acquire knowledge which may be used as a basis of new knowledge. It is that spirit in which our Universities in England are so lamentably deficient; it is that spirit which has contributed to the success of the Teutonic nations, and which is beginning to influence the United States. For this condition of things our examinational system is largely to

blame; originally started to remedy the abuses of our Civil Service, it has eaten into the vitals of our educational system like a canker; and it is fostered by the farther abuse of awarding scholarships as the results of examinations. The pauperisation of the richer classes is a crying evil; it must some day be cured. Let scholarships be awarded to those who need them; not to those whose fathers can well afford to pay for the education of their children. 'Pot-hunting' and Philosophy have absolutely nothing in common.

There are some who hold that the time of an investigator is wasted in teaching the elements of his subject. I am not one of those who believe this doctrine, and for two reasons:—first, it is more difficult to teach the elements of a subject than the more advanced branches; one learns the tricks of the trade by long practice; and the tricks of this trade consist in the easy and orderly presentment of ideas. And it is the universal experience that senior students gain more good from instruction in advanced subjects by demonstrators than juniors would in elementary subjects. For the senior student makes allowances; and the keenness and interest of the young instructor awakens *his* interest. Second, from the teachers' point of view, it is always well to be obliged to go back on fundamentals. One is too apt, without the duty of delivering elementary lectures, to take these fundamentals for granted; whereas, if they are recapitulated every year, the light of other knowledge is brought to bear on them, and they are given their true proportion; indeed, ideas occur which often suggest lines of research. It is really the simplest things which we know least of; the atomic theory; the true nature of elasticity; the cause of the ascent of sap in plants; the mechanism of exchange in respiration and digestion; all these lie at the base of their respective sciences, and all could bear much elucidation. I believe

therefore, that it is conducive to the furtherance of knowledge that the investigator should be actively engaged in teaching. But he should always keep in view the fact that his pupils should themselves learn how to investigate; and he should endeavour to inculcate that spirit in them.

It follows that the teachers in the Philosophical Faculty should be selected only from those who are themselves contributing to the advancement of knowledge; for if they have not the spirit of research in them how shall they instil it into others? It is our carelessness in this respect (I do not speak of University College, which has always been guided by these principles, but of our country as a whole) which has made us so backward as compared with some other nations. It is this which has made the vast majority of our statesmen so careless, because so ignorant, of the whole frame of mind of the philosopher; and which has made it possible for men high in the political estimation of their countrymen to misconceive the functions of a University. It is true that one of these functions of a University is to 'train men and women fit for the manifold requirements of the Empire'; that we should all heartily acknowledge; but no man who has any claim to university culture can possibly be contented if the University does not annually produce much work of research. It is its chief excuse for existence; a University which does not increase knowledge is no University; it may be a technical school; it may be an examining board; it may be a coaching establishment; but it has no claim to the name University. The best way of fitting young men for the manifold requirements of the Empire is to give them the power of advancing knowledge.

It may be said that many persons are incapable of exhibiting originality. I doubt it. There are many degrees of originality, as there are many degrees of

rhyming, from the writer of doggerel to the poet, or many degrees of musical ear, from the man who knows two tunes, the tune of 'God save the King' and the *other* tune, to the accomplished musician. But in almost all cases, if caught young, the human being can be trained more or less; and, as a matter of fact, natural selection plays its part. Those young men and women who have no natural aptitude for such work—and they are usually known by the lack of interest which they take in it—do not come to the University. My experience is that the majority, or at least a fair percentage of those who do come, possess germs of the faculty of originating, germs capable of development, in many instances, to a very high degree. It is such persons who are of most value to the country; it is from them that advance in literature and in science is to be expected; and many of them will contribute to the commercial prosperity of the country. We hear much nowadays of technical education; huge sums of money are being annually expended on the scrappy scientific education in evening classes of men who have passed a hard day in manual labour, men who lack the previous training necessary to enable them to profit by such instruction. It may be that it is desirable to provide such intellectual relaxation; I even grant that such means may gradually raise the intellectual level of the country; but the investment of money in promoting such schemes is not the one likely to bear the most immediate and remunerative fruit. The Universities should be the technical schools; for a man who has learned to investigate can bring his talents to bear on any subject brought under his notice, and it is on the advance, and not the mere dissemination of knowledge, that the prosperity of a country depends. To learn to investigate requires a long and a hard apprenticeship; the power cannot be acquired by an odd hour spent now and again; it is as

difficult to become a successful investigator as a successful barrister or doctor, and it requires at least as hard application and as long a period of study.

I do not believe that it is possible for young men or women to devote sufficient time during the evening to such work. Those who devote their evening hours to study and investigation do not bring fresh brains to bear on the subject; they are already fatigued by a long day's work; and, moreover, it is the custom in most of the colleges which have evening classes to insist upon their teachers doing a certain share of day work; they, too, are not in a fit state to direct the work of their pupils nor to make suggestions as to the best method of carrying it out. Moreover, the official evening class is from seven to ten o'clock, and for investigation in science a spell of three hours at a time is barely sufficient to carry out successfully the end in view; indeed, an eight hours' day might profitably be lengthened into a twelve hours' day, as it not infrequently is. It is heartrending in the middle of some important experiment to be obliged to close and postpone it till a future occasion, when much of the work must necessarily be done over again.

These are some of the reasons why I doubt whether University education, in the Philosophical Faculty at least, can be successfully given by means of evening classes.

Although my work has lain almost entirely in the domain of science, I should be the last man not to do my best to encourage research in the sphere of what is generally called 'arts.' In Germany of recent years a kind of institution has sprung up which is termed a *Seminar*. The word may be translated a 'literary laboratory.' I will endeavour to give a short sketch on the way in which these literary laboratories are conducted. After the student has attended a course of lectures on the subjects

to which he intends to devote himself, and is ripe f research, he enters a Seminar, in which he is provide with a library, paper, pens and ink, and a subject. Tl method of using the library is pointed out to him, and l is told to read books which bear on the particular subje in question; he is made to collate the information whic he gains by reading, and to elaborate the subject whic is given him. Naturally his first efforts must be crude, b *ce n'est que le premier pas qui coûte.* It probably costs hi blame at the hands of his instructor; after a few unsu cessful efforts, however, if he has any talent for the pa ticular investigation to which he has devoted himse his efforts improve and at last he produces somethi respectable enough to merit publication. Thus he exposed to the criticism of those best competent to judg and he is launched in what may be a career in Historica Literary, or Economic research.

Such a Seminar is carried on in philological a linguistic studies, in problems of economy involvi statistics, in problems of law involving judicial decisio and of history in which the relations between the develo ment of the various phases in the progress of nations traced. The system is borrowed from the well-know plan of instruction in a physical or chemical laborato Experiments are made in literary style. These expe ments are subjected to the criticism of the teacher, a thus the investigator is trained. But it may be object that the youths who frequent our Universities have not sufficient knowledge of facts connected with such subjec to be capable of at once entering on a training of th kind. That may be so; if it is the case, our schools mu look to it that they provide sufficient training. Ev under present circumstances, however, I do not think am mistaken in supposing that a young man or wom who enters a University at the age of eighteen years wi

the intention of spending three years in literary or historical studies will not at the end of the second year be more benefited by a course at the Seminar, even though it should result in no permanent addition to literature or history, than if he were to spend his time in mere assimilation. It is not the act of gaining knowledge which profits, it is the power of using it, and while in order to use knowledge it is necessary to gain it, yet a training in the method of using knowledge is much more important and profitable than a training in the method of gaining it. I do not know whether there exists in this country a single example of the continental Seminar; there was some talk of founding such a literary laboratory in University College, but, as usual, the attempt was frustrated by a lack of funds; the attempt would also have been frustrated by the requirements of the present system of examination in the University of London; but there is, fortunately, good hope of changing that system and of developing the minds of students on those lines which have proved so fruitful where they have been systematically followed.[1]

There is one subject, of which the votaries are so few, that it is difficult to treat in the same manner as those literary and scientific subjects of which I have been speaking; that subject is mathematics. While many persons have a certain amount of mathematical ability which they cultivate as a means to an end, those who are born mathematicians are as few as those who are born musicians. I have had the privilege of discussing this question with one of the foremost mathematicians of Europe—Professor Klein of Göttingen. He tells me that while he is content for the most part to treat mathematics as a technical study, imparting to his pupils so much as is necessary for them to use it easily as an instrument, he

[1] Several Seminars have now been started at University College (Sept. 1908).

discourages young men, unless they are especially endowed by Nature, from pursuing the study of mathematics with the object of cultivating a gift for that subject. Especially gifted men occasionally turn up, and those who possess mathematical insight are able to profit by the instruction of the professor in developing some special branch of the subject. Mathematical problems, he tells me, are numerous, but they demand such an extensive knowledge of what has already been done that very few persons who do not devote their lives to the subject are able to cope with them, and it is only those who are born with a mathematical gift who can afford to devote their lives to mathematics, for the standard is high, and the prizes are few.

Many, I suppose, who are at present listening to me would be disappointed were I not to refer to the functions of a University with reference to examinations. A long course of training, lasting now for the best part of seventy years, has convinced the population of London that the chief function of a University is to examine. Believe me, the examination should play only a secondary part in the work of a University. It is necessary to test the acquirements of the students whom the teachers have under their charge, but the examination should play an entirely subordinate part. To be successful in examinations is unfortunately too often the goal which the young student aims at, but it is one which all philosophical teachers deprecate. To infuse into his pupils a love of the subject which both are at the same time teaching and learning, is the chief object of an enthusiastic teacher; there should be an atmosphere of the subject surrounding them —an umbra—perhaps I should call it an aura; for it should exert no depressing influence upon them. The object of both classes of students (for I count the teacher a student) should be to do their best to increase know-

ledge of the subject on which they are engaged. That this is possible, many teachers can testify to by experience; and it is the chief lesson learned by a sojourn in a German laboratory. Where each student is himself engaged in research, interest is taken by the students in each others' work; numerous discussions are raised regarding each questionable point; and the combined intelligence of the whole laboratory is focussed on the elucidation of some difficult problem. There is nothing more painful to witness than a dull and decorous laboratory, where each student keeps to his own bench, does not communicate with his fellow-students, does not take an interest in their work, and expects them to manifest no interest in his. It is only by friction that heat can be produced, and heat, by increasing the frequency of vibration, results, as we know, in light.

The student should look forward to his examination not as a solemn ordeal which he is compelled to go through with the prospect of a degree should he be successful, but as a means of showing his teacher and his fellows how much he has profited by the work which he has done; those who pursue knowledge in this spirit and those, be it remarked, who examine in this spirit will look forward to examination with no apprehension; not, perhaps, with joy, for after all it is a bore to be examined and perhaps a still greater bore to examine, but it is a necessary step for the student in gaining self-assurance and the conviction of having profited by his exertions; and for the teacher, as a means of insuring that his instruction has not been profitless to his student. In this connection I cannot refrain from remarking, that that genius for competition which has over-ridden our nation of England, appears to me to be misplaced. Far too much is thought of the top man; very likely the second or even the tenth, or it may be the fiftieth has a firmer

grasp of his subject, and in the long-run would display more talent. Let us take comfort, however, in the thought, that the day of examinations, for the sake of examinations, is approaching an end.

It may surprise many to learn that the suggestion that in England teachers do not usually examine their own pupils for degrees, is, abroad, received in a spirit of surprise not unmixed with incredulity. Americans and Germans to whom I have mentioned this state of matters cannot realise that the teacher is not considered fit to be trusted to examine his own pupils, and, singular to state, they maintain that no one else can possibly do so with any attempt at fairness; it appears to them, as it appears to me, an altogether untenable position to hold that a man selected to fill an important professorship, after many years' trial in a junior position, should be suspected of such (shall I say) ambiguous ideas regarding common honesty, that he will always arbitrate unfairly in favour of his own pupils. Such a supposition is an insult to the professor; and the exclusion of the teacher elevates examination to the position of a fetish; it is that, together with the spirit of emulation and competition, which has done so much to ruin our English education. The idea of competitive examination is so ingrained in the minds of Englishmen, that it is difficult for them to realise that the object of a University is not primarily to examine its pupils, but to teach them to teach themselves; and also they have still to acquire the conviction that students should be found not merely among the *alumni* of the University but also among all members of the staff. The spirit which should prevail with us should be the spirit of gaining knowledge—gaining knowledge not for the satisfaction of one's own sense of acquisitiveness, but in order to be able to increase the sum-total of what is known. All should work together, senior and junior staff

graduates and undergraduates, in order to diminish man's ignorance.

To sum up. As it exists at present, a University is a technical school for theology, law, medicine, and engineering. It ought to be also a place for the advancement of knowledge, for the training of philosophers, of those who love wisdom for its own sake; and while as a technical school it exercises a useful function in preparing many men and women for their calling in life, its philosophical faculty should impart to those who enter its halls that faculty of increasing knowledge which cannot fail to be profitable not only to the intellect of the nation, but also to its industrial prosperity. I regard this as the chief function of a University.

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